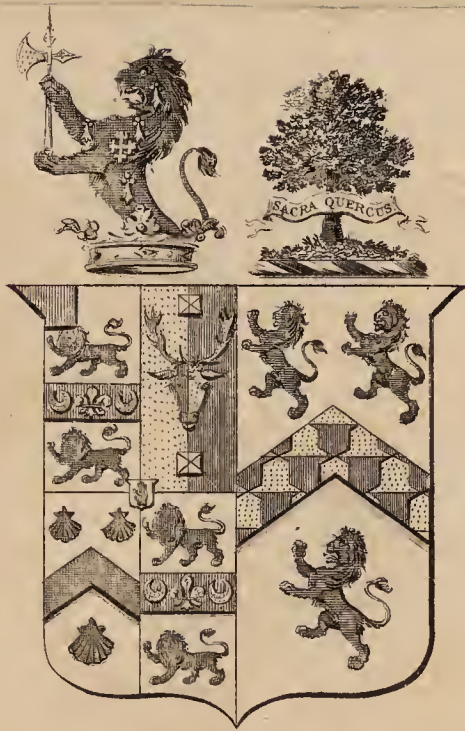


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Sir Francis L. H. Goodricke Bart

E X P E R I M E N T S

U P O N

V E G E T A B L E S,

D I S C O V E R I N G

Their great Power of purifying the
Common Air in the Sun-shine,

A N D O F

Injuring it in the Shade and at Night.

T O W H I C H I S J O I N E D,

A new Method of examining the accurate
Degree of Salubrity of the Atmosphere.

By JOHN INGEN-HOUSZ,

Counsellor of the Court and Body Physician
to their IMPERIAL and ROYAL MAJESTIES,
F. R. S. &c. &c.

L O N D O N:

Printed for P. E L M S L Y, in the Strand;
and H. P A Y N E, in Pall Mall. 1779.

1871

JOHN PRINGLE
Esq.
Member of the
Council of the
Institution

A
CHARTERED
Institution
The
Institution
It is not in the power of a
to make a return
reference
Institution



T O

Sir JOHN PRINGLE, Bart.

Physician to his Britannic Majesty,
late President of the Royal Society,
Member of the Royal Academy of Paris, &c. &c.

S I R,

A GRATEFUL remembrance of
past services is as just a tribute
due to those from whom they
are received as the acquitting of a
debt contracted in any other manner.
If it is not in the power of a man
to make a return suitable to the be-
nefits received, he is, however, in

a 2

duty

duty bound to shew, by the best method in his power, a thankful heart to his benefactor.

Ingratitude was by the ancient Greeks held as a crime of the blackest dye, as tending directly to destroy the motives of mutual benevolence, and to dissolve the ties of friendship, that source of human happiness, without which life itself is scarcely worth enjoying.

The ungrateful, conscious of his misbehaviour, and looking upon his benefactor as upon a judge who has pronounced a just and severe sentence upon him, endeavours to find reason for breaking off with him ; while his benefactor, looking upon the ungrateful as upon a monster unworthy

unworthy of his regard, is induced to shut, for the future, his heart against others.

No man upon earth can have stronger reasons for a due sense of gratitude than I acknowledge to you. You bestowed many civilities upon me, who had never been in the way of doing you any service whatever. You granted me your friendship almost as soon as I was acquainted with you. You encouraged my eagerness for improving myself in medical knowledge, by communicating to me what you had learned by a laborious life; by that experience which an assiduous and most attentive zeal, bestowed in the care of the great military hospitals in

the time of war, and a most successful private practice, had afforded you, and of which your celebrated work upon *The Diseases of the Army* will be an honourable and everlasting testimony, as well as a real benefit, to the latest posterity.

You always gave me, with the greatest sincerity, your advice in what manner, and in whose company, I could most improve myself in the various branches of medicine and natural knowledge which I took a delight to cultivate.

It was you, SIR, who, among those many respectable and learned men, whose constant friendship towards me has made an indelible and grateful impression on my mind,

con-

contributed principally to that particular happiness I enjoyed during so many years in this island; that felicity which a free and independent man finds in the pursuit of knowledge and wisdom in the society and friendly intercourse of those who have distinguished themselves by their learning.

But, SIR, among the many obligations which I owe you, there is one of such importance, that the very thought of it strikes me with reverence and with the deepest sense of gratitude for you. You did me a service which I cannot forbear to mention; though I know that your modesty would hardly permit me to

express the true situation of my mind in that respect.

Permit me, SIR, to leave behind me some public testimony of my respectful gratitude to you, as the only, though small, return I can make you; the only way by which I can publicly shew, that the unsolicited favours so generously bestowed upon a foreigner, who could not claim the least merit with you, have made so strong an impression on my mind as no time is able to weaken. You have recommended me, SIR, without my soliciting any favour from you, to those August Sovereigns who are still the support of the illustrious HOUSE OF AUSTRIA; those

those powerful Monarchs whose graciousness, benevolence, and magnanimity, equal the supreme grandeur of their station. These August Sovereigns, after having suffered so many repeated losses by that dreadful disease the Small-pox, resolved at last to check that terrible havock in their illustrious Family, and ordered their Ambassador to send to their Court a physician from this island, capable of fulfilling the important trust of saving, by means of inoculation, the remainder of the Royal Offspring, which had as yet escaped the infection. Being consulted on the choice of a proper person, you proposed me without hesitation, and thus
opened

[*]

opened to me a wide door to emoluments and honours.

After having been so publicly and so honourably called from a distant country to the most generous and powerful Monarchs; and after having contributed to the tranquillity and happiness of so many illustrious Princes, who, being educated under the maternal care of the most virtuous PRINCESS, are become highly important to mankind, and have filled the world with a well-founded confidence to see its happiness promoted by their means; whatever advantage or reputation I have acquired from such a flattering appointment,

pointment, I derive it all from your friendship.

My earnest desire of not quitting this country without leaving you some public testimony of my real sentiments towards you, excited me to hurry this work to the press without having time enough to finish it as I desired. If it had been in my power to have spent the ensuing winter in this country, I might possibly have made it more worthy your patronage, and of appearing in the world under your auspices. I present it to you imperfect as it is; and beg of you to look upon it only as a public mark of my respect and gratitude, which I shall retain in full force

force to the end of my life, and
with which I have the honour of
subscribing myself,

S I R,

Your very much obliged

and faithful friend and servant,

J. INGEN-HOUSZ.

London,
October 12, 1779.

P R E F A C E.

THE common air, that element in which we live, that invifible fluid which furrounds the whole earth, has never been fo much the object of contemplation as it has in our days: it never engaged fo much the attention of the learned as it has of late years. This fluid, diffufed every where, *the breath of life*, deserves fo much the more the attention and investigation of philosophers, as it is the only fubftance without which we can fcarce fubfift alive a fingle moment, and whose good or bad qualities have the greateft influence upon
our

our constitution. The most active poisons which are known do not so quickly destroy the life of an animal as the want of air, or the breathing of it when it is rendered highly noxious. It will appear in this work, that those very plants, which, influenced by the light of the sun, repair the injury done to this fluid by the breathing of animals, and by many other causes, may, in different circumstances, poison so much this very element, as to render it absolutely unfit for respiration, and, instead of keeping up life, to extinguish it in a moment. Therefore this universally-diffused element deserves not only the pursuit of philosophers, but claims more immediately the attention of those whose profession it is to preserve health and to cure diseases. I have bestowed

bestowed some labour upon this subject, both as a philosopher and as a physician.

When I first found in the works of that excellent philosopher and inventive genius, the reverend Dr. Priestley, his important discovery, that plants wonderfully thrive in putrid air; and that the vegetation of a plant could correct air fouled by the burning of a candle, and restore it again to its former purity and fitness for supporting flame, and for the respiration of animals; I was struck with admiration: and I could not read afterwards, but with a kind of extasy, the application which Sir John Pringle made of this discovery in his elaborate discourse, delivered at the Royal Society in November 1773, when he conferred, as president of that learned

learned Body, the annual prize medal upon Dr. Priestley, decreed to him as an honourable testimony of their approbation of the successful labours bestowed by him upon the doctrine of air. “ From these dis-
 “ veries,” says he, “ we are assured,
 “ that no vegetable grows in vain,
 “ but that, from the oak of the forest
 “ to the grass in the field, every in-
 “ dividual plant is serviceable to
 “ mankind; if not always distin-
 “ guished by some private virtue,
 “ yet making a part of the whole,
 “ which cleanses and purifies our
 “ atmosphere. In this the fragrant
 “ rose and deadly night-shade co-
 “ operate: nor is the herbage, nor
 “ the woods that flourish in the
 “ most remote and unpeopled re-
 “ gions, unprofitable to us, nor we
 “ to them; considering how con-
 “ stantly

“stantly the winds convey to them
 “our vitiated air for our relief, and
 “for their nourishment. And if
 “ever these salutary gales rise to
 “storms and hurricanes, let us still
 “trace and revere the ways of a be-
 “neficent Being, who not fortui-
 “tously, but with design, not in
 “wrath, but in mercy, thus shakes
 “the waters and the air together, to
 “bury in the deep those putrid and
 “pestilential effluvia which the ve-
 “getables upon the face of the
 “earth had been insufficient to con-
 “sume.”

Since I read that elegant discourse,
 I have wished that some industrious
 philosopher would bestow his labour
 in tracing Nature in its operations,
 and in discovering the manner in
 which the vegetable kingdom is
 subservient to the animal, in cor-
 b recting

recting the mass of air contaminated by their respiration, and perhaps too by their perspiration. The following pages will shew, whether the pains I took, in the course of this summer, to investigate this important subject, have been attended with some degree of success. I am far from thinking that I have discovered the whole of this salutary operation of the vegetable kingdom; but I cannot but flatter myself, that I have at least proceeded a step farther than others, and opened a new path for penetrating deeper into this mysterious labyrinth.

Among the various useful discoveries with which Dr. Priestley has already enriched, and still continues to enrich, natural knowledge, none, in my opinion, are of more importance

ance than those he made upon the various kinds of airs.

The discovery of that wonderful aerial fluid, which in purity and fitness for respiration so far exceeds the best atmospheric air, that an animal may protract its life in it five times longer than in the best common air, excites so much the greater admiration, as he found it first in such bodies which by their nature must have been suspected to conceal rather within their substance deleterious qualities, such as calcined mercury and red precipitate. He has given to this air the very proper appellation of *dephlogisticated air*, or air deprived of that inflammable principle which is the chief ingredient that renders our atmospheric air more or less impure, and thus more or less fit for respiration.

His discovery of that peculiar quality which nitrous air possesses, of destroying or being destroyed by common air in proportion to its purity, is one of those inventions whose utility will be more and more conspicuous, when it shall have undergone all the improvements of which it is susceptible. Let it be mentioned to his honour, that his candour and modesty have made him under-rate the value of this useful production of his inquiries, when he says, in his last work, intitled, *Experiments and Observations relating to various Branches of Natural Philosophy, with a Continuation of the Observations of Air*, p. 269,

“ When I first discovered the pro-
 “ perty of nitrous air as a test of
 “ the wholesomeness of common
 “ air, I flattered myself that it might
 “ be

“ be of considerable practical use;
 “ and, particularly, that the air of
 “ distant places and countries might
 “ be brought and examined together
 “ with great ease and satisfaction:
 “ but I own, that hitherto I have
 “ rather been disappointed in my
 “ expectation from it.” And he
 concludes thus: “ I have frequently
 “ taken the open air in the most
 “ exposed places in this country, at
 “ *different times of the year*, and in
 “ *different states of the weather*,
 “ &c.; but never found the differ-
 “ ence so great as the inaccuracy,
 “ arising from the method of
 “ making the trial, might easily
 “ amount to or exceed.”

Since I saw the manner of putting
 different airs to the nitrous test,
 which Abbé Fontana now makes
 use of, and which I have in my in-

quiries for the most part imitated, I cannot but think more favourably of the importance of this discovery than the author himself does. I even think with the Abbé, that, by using convenient and accurate instruments, and by observing to the greatest nicety all the manoeuvres of the operation constantly in the same way, we may with as much precision judge of the degree of purity of common air, as we now are able to judge of its degree of heat and cold by a good thermometer.

Indeed, by this method, even all the changes which the constitution of the atmosphere undergoes daily, in the same place, are observed with so much accuracy, that, by making ten observations with the same air, the difference will scarcely amount to

to $\frac{1}{500}$ th of the two airs employed in the experiment.

The discovery of Dr. Priestley, that plants thrive better in foul air than in common and in dephlogisticated air, and that plants have a power of correcting bad air, has thrown a new and important light upon the arrangement of this world. It shews, even to a demonstration, that the vegetable kingdom is subservient to the animal; and, *vice versa*, that the air, spoiled and rendered noxious to animals by their breathing in it, serves to plants as a kind of nourishment. But in what manner this faculty of the plants is excited remained still unknown.

There was even some doubt left in the mind of many philosophers, whether the facts related by Dr. Priestley were not owing to some

particular accident, as they had by no means been uniform; nay, had even been often contradictory, as he himself candidly owns (see vol. I. p. 91, &c. of Dr. Priestley's work on the subject of air, and his last work, p. 296.); and as Mr. Sheele had constantly observed a contrary effect from beans.

Dr. Priestley acknowledges, p. 299. that, by repeating (1778) again his experiments, *they proved to be unfavourable to his former hypothesis.* “For,” says he, “whether I made the experiments with
“air injured by respiration, the
“burning of candles, or any other
“phlogistic process, it did not grow
“better but worse; and the longer
“the plants continued in the air,
“the more phlogisticated it was.”
He proceeds thus farther: “I have
“had

“ had several instances of the air
 “ being undoubtedly meliorated by
 “ this process, especially by the
 “ shoots of strawberries, and some
 “ other plants, which I could, by
 “ bending, introduce into the jars
 “ or phials of air supported near
 “ them in the garden, when the
 “ roots continued in the earth.—
 “ I had other instances, no less un-
 “ questionable, of common air not
 “ only receiving no injury, but even
 “ considerable advantage, from the
 “ process, having been rendered in
 “ some measure dephlogisticated by
 “ it, so as to be much more dimi-
 “ nished by nitrous air than before,
 “ a thing which I was far from ex-
 “ pecting.—In most of the cases in
 “ which the plants failed to me-
 “ liorate the air, they were either
 “ manifestly sickly, or at least did
 “ not

“ not grow and thrive, as they did
 “ most remarkably in my first ex-
 “ periments at Leeds, the reason of
 “ which I cannot discover. — In
 “ those instances in which the plants
 “ grew the best, they were, how-
 “ ever, but sickly, as appeared by the
 “ leaves soon turning yellow, and
 “ falling off when the least motion
 “ was given to them. In some
 “ cases, however, as in those men-
 “ tioned in vol. I. p. 91, I saw no
 “ particular reason why the air
 “ should not have been meliorated.

“ Upon the whole, I still think
 “ it *probable*, that the vegetation
 “ of healthy plants, growing in si-
 “ tuations natural to them, has a
 “ salutary effect on the air in which
 “ they grow. For one clear in-
 “ stance of the melioration of air in
 “ these circumstances should weigh
 “ against

“ against a hundred cafes, in which
 “ the air is made worfe by it.”

Soon after, p. 305, he relates feveral instances in which a plant had, in the fpace of feven, eight, ten, or more days, effectually mended the foul air in which it was made to grow. P. 309, he relates a fact, in which a fprig of *winter favory*, kept growing in a jar from the 16th of June to the 20th, had improved the air evidently, which improvement he found by three repeated trials to be in the proportion of 1.275 to 1.375. He relates another instance, in which air was fo much improved by a fprig of parfley growing in it, from the 16th of June to the 1ft of July, that one meafure of it with one of nitrous air occupied only one meafure.

After

After all, he concludes with the following words, p. 310: “ When
 “ these observations are well confi-
 “ dered, I think it will hardly be
 “ doubted but that there is some-
 “ thing in the process of vegetation,
 “ or at least something usually *at-*
 “ *tending* it, that tends to meliorate
 “ the air, in which it is carried on,
 “ whatever be the *proximate cause*
 “ of this effect, whether it be the
 “ plants imbibing the phlogistic
 “ matter, as part of their nourish-
 “ ment, or whether the phlogiston
 “ unites with the vapour that is
 “ continually exhaled from them ;
 “ though of the two opinions I
 “ should incline to the former.”

Mr. *Sheele* is so far from thinking
 that air is meliorated by plants,
 that he even maintains, that vege-
 tation has the same effect on air
 that

that respiration has. He allows, however, that plants do not grow so well in dephlogisticated as in common air.

At the end of Section XXXIII, in which he treats of the *spontaneous emission of dephlogisticated air from water in certain circumstances*, he speaks thus: “ It will probably be
 “ imagined, that the result of the
 “ experiments recited in this Sec-
 “ tion throws some uncertainty on
 “ the result of those recited in this
 “ volume, from which I have con-
 “ cluded, that air is meliorated by
 “ the vegetation of plants, especially
 “ as the water, by which they were
 “ confined, was exposed to the open
 “ air, and the sun, in a garden.
 “ To this I can only say, that I was
 “ not then aware of the effect of
 “ these circumstances, and that I
 “ have

“ have represented the naked *facts*
 “ as I observed them ; and, having
 “ no great attachment to any par-
 “ ticular *hypothesis*, I am very will-
 “ ing that my reader should draw
 “ his own conclusions for himself.”

Dr. Priestley, having observed that
 bubbles of air seemed to issue spon-
 taneously from the stalks and roots
 of several plants kept in the water,
 suspected immediately, that perhaps
 this air, if found better than com-
 mon air, had been percolated through
 the plant, and purified by leaving
 its phlogiston in the plant as its
 nourishment. With this view he
 plunged many phials containing
 sprigs of mint in water, laying
 them in such a manner, as that
 any air, which might be discharged
 from the roots, would be retained
 in the phials, the bottoms being
 a little

a little elevated. In this position the sprigs of mint grew very well, and in some of the phials he observed a quantity of air to be collected, though very slowly ; but he was much disappointed, that some of the most vigorous plants produced no air at all. At length, however, from about ten plants he collected, in the course of a week, about half an ounce-measure of air, which he found so pure, that one measure of it and one of nitrous air occupied the space of only one measure.

This remarkable fact contributed not a little to confirm his faith in the hypothesis of the purification of the atmosphere by vegetation ; but he did not enjoy this satisfaction long ; for, as he found that other plants of the same species

produced

produced no such effect, and that, what he thought more extraordinary, the phials, in which the above mentioned plants had grown, the inside of which were covered with a green kind of matter, continued to yield air as well when the plants were out of them as they had done before ; he was convinced, that the plants had not, as he had imagined, contributed any thing to the production of this pure air. See Dr. Priestley's last work, p. 337 and 338.

Thus far this matter was carried on when I took it up in June last. I must acknowledge, that, from what is above related from Dr. *Priestley's* works, I had little doubt but there was some quality in plants proper for correcting bad air, and improving ordinary air. My curiosity led me to investigate in what manner
this

this operation is carried on, whether the plants mend air by abforbing, as part of their nourishment, the phlogiftic matter, and leaving thus the remainder of the air pure (to which opinion Dr. Priestley inclines the moft) ; or whether perhaps the plants poffefs fome particular virtue hitherto unknown, by which they change bad air into good air, and good into better, which I fufpected to be the cafe.

I was not long engaged in this enquiry before I faw a moft important fcene opened to my view : I obferved, *that plants not only have a faculty to correct bad air in fix or ten days, by growing in it, as the experiments of Dr. Priestley indicate, but that they perform this important office in a compleat manner in a few hours ; that this wonderful operation*

is by no means owing to the vegetation of the plant, but to the influence of the light of the sun upon the plant: I found that plants have, moreover, a most surprizing faculty of elaborating the air which they contain, and undoubtedly absorb continually from the common atmosphere, into real and fine dephlogisticated air; that they pour down continually, if I may so express myself, a shower of this depurated air, which, diffusing itself through the common mass of the atmosphere, contributes to render it more fit for animal life; that this operation is far from being carried on constantly, but begins only after the sun has for some time made his appearance above the horizon, and has, by his influence, prepared the plants to begin anew their beneficial operation upon the air, and thus upon the animal creation, which

which was stopt during the darkness of the night ; that this operation of the plants is more or less brisk in proportion to the clearness of the day, and the exposition of the plants more or less adapted to receive the direct influence of that great luminary ; that plants shaded by high buildings, or growing under a dark shade of other plants, do not perform this office, but, on the contrary, throw out an air hurtful to animals, and even contaminate the air which surrounds them ; that this operation of plants diminishes towards the close of the day, and ceases entirely at sun-set, except in a few plants, which continue this duty somewhat longer than others ; that this office is not performed by the whole plant, but only by the leaves and the green stalks that support them ; that acrid, ill-scented,

and even the most poisonous plants perform this office in common with the mildest and the most salutary; that the most part of leaves pour out the greatest quantity of this dephlogisticated air from their under surface, principally those of lofty trees; that young leaves, not yet come to their full perfection, yield dephlogisticated air less in quantity, and of an inferior quality, than what is produced by full-grown and old leaves; that some plants elaborate dephlogisticated air better than others; that some of the aquatic plants seem to excel in this operation; that all plants contaminate the surrounding air by night, and even in the day-time in shaded places; that, however, some of those which are inferior to none in yielding beneficial air in the sun-shine, surpass others in the power

power of infecting the circumambient air in the dark, even to such a degree, that in a few hours they render a great body of good air so noxious, that an animal placed in it loses its life in a few seconds; that all flowers render the surrounding air highly noxious, equally by night and by day; that the roots removed from the ground do the same, some few, however, excepted; but that in general fruits have the same deleterious quality at all times, though principally in the dark, and many to such an astonishing degree, that even some of those fruits which are the most delicious, as, for instance, peaches, contaminate so much the common air, as would endanger us to lose our lives, if we were shut up in a room in which a great deal of such fruits are stored up; that the sun by
 c 3 itself

itself has no power to mend air without the concurrence of plants, but on the contrary is apt to contaminate it.

These are some of the secret operations of plants I discovered in my retirement, of which I will endeavour to give some account in the following pages ; submitting, however, to the judgement of the candid reader the consequences, which I thought might fairly be deduced from the facts I am to relate.

I must not omit to acquaint the reader, that, in pursuing the experiments related in this work, he will find that he labours in vain, if he does not make use of pump-water freshly drawn ; for if this water has been exposed to the open air during some time, it will have parted with

a great deal of its own air, and will therefore be apt to absorb the air from the leaves. It may also happen, that every pump-water may not be found equally as good as that which I met with in my country dwelling, though as yet I have no positive reason to think so; but I have some grounds to believe, that water drawn from an open well is far inferior in goodness to that which is forced up by a pump, as the former is too much exposed to the open air.

By casting an eye upon the experiments related in this work, it will be easily understood, why, in every experiment of this kind, some difference in the result will commonly be observed; for the peculiar degree of goodness of the dephlogisticated air obtained from

the leaves depends upon too many circumstances to be constantly of the same quality. Some more or less light of the sun thrown upon the jar will make some difference; the leaves being more or less crowded together, will make a remarkable difference, as a great number of them may be shaded from the sun by others.

As I made the greatest part of my experiments according to the present method of proceeding of my respectable friend the Abbé Fontana, it would have been difficult to imitate the experiments related in this work, and even to understand the manner in which they were made, if he had not given me leave to anticipate the publication of his own ingenious contrivance, and of his present method

thod of putting the different species of air to the test. This kindness of that gentleman deserves my public thanks.

Inaccuracies in the manner of expressing myself will find some indulgence in a man born and educated in the Republic of the United Provinces, and who was not early in life acquainted with the English language.

The AUGUST SOVEREIGNS, whom I have the honour to serve, condescending graciously to prolong my leave of absence, and allowing me to spend the present summer in this island, I thought it my duty to apply the time granted me by their goodness to an useful purpose, and to make all the advantage I could of that peculiar degree of health which

I have

I have always enjoyed in this climate.

On purpose to avoid every cause of obstructing my mind in the close pursuit of the object I had in view, and in tracing Nature in its operation on this subject, I disengaged myself from the noise of the metropolis, and retired to a small villa, where I was out of the way of being interrupted by any body in the contemplation of Nature.

This work is a part of the result of above 500 experiments, all which were made in less than three months, having begun them in June, and finished them in the beginning of September, working from morning till night. From these experiments some more consequences might have been drawn, if I had had more time to employ myself
in

in a work upon such important matter. Whatever I have been able to deduce from my labours is done in a hasty manner, as my stay in this country was far too limited to allow me to compose my work in a regular and more satisfactory manner.

Though I was very far from foreseeing all the discoveries which I made in the course of this summer, yet I was persuaded that a good deal of the oeconomy of the vegetable kingdom might be discovered by a steady pursuit of experiments tending to trace the operations of Nature. I had this object in view some years ago; but, as I did not enjoy such a favourable disposition of mind and body as was necessary for a task, in which all possible steadiness, perseverance,

verance, and close attention were requisite, I deferred the undertaking till I should find myself more fit for it.

Detached experiments may indeed be very useful when a sufficient number is collected to draw some conclusions from them; but, without pursuing methodically the same object, discoveries are to be expected only by mere chance, and are even sometimes overlooked. I owe to the example of my worthy friend, the Abbé Fontana, the thorough persuasion, which I now entertain, that natural knowledge can make but a very slow progress in the hands of those who have not patience and assiduity enough in pursuing one and the same object, till they discover some things undiscovered before; or till they find
that

that the difficulty of the undertaking surpasses their abilities.

WHEN this book was entirely printed, and nothing but the latter end of the preface unfinished, I was informed by my friend the Abbé Fontana, that he discovered a few days ago a new method of procuring to a sick person the benefit of breathing any quantity of dephlogisticated air at a cheap rate.

This very year a paper of mine was read before the Royal Society, and ordered for the press (containing a new theory of the effects of gunpowder, and the discovery of a new and powerful explosive air), in which I say, that the rapid progress our modern philosophers daily

make on the different kinds of air, induce me to believe that we touch at the happy moment, at which a very easy and little expensive method of producing this beneficial fluid, in any quantity wanted, will be produced for the cure of many diseases.

I have the great satisfaction to inform the reader, that my prediction is fulfilled even before it hath reached the public, and that this important *desideratum* in medicine is discovered.

Abbé Fontana found that an animal breathing in either common or dephlogisticated air renders it unfit for respiration by communicating to it a considerable portion of fixed air, which is generated in our body, and thrown out by the lungs as excrementitious. This fixed air is easily
 absorbed

absorbed by shaking it in common water, but infinitely more readily by the contact with quick-lime water.

He fills one of the large receivers of an air-pump, which are very wide at their upper extremity, half full of dephlogisticated air extracted from nitre, so that it may contain about 500 cubic-inches of this air, which will serve for breathing during half an hour. The manner of drawing this air out of the receiver, is either by thrusting a bended glass tube under the receiver (when this is floating in water, in which it is supported by its peculiar bulky form), reaching into the air itself, and keeping the other extremity in the mouth; thus drawing this air in the lungs, and breathing it out by the same tube. This air returning from
the

the lungs is infected by fixed air, which being immediately absorbed by the contact with lime-water, the dephlogisticated air is restored very near to its former purity. Instead of the bended tube just mentioned, the top of the receiver may be made as the neck of a bottle, and a tube may be fixed to it, having a cock to open and shut the passage as required.

We consume, by each inspiration, about 30 cubic-inches of air; and thus, allowing 15 inspirations for a minute, we consume each minute 450 cubic-inches of air. The Abbé Fontana found, that the dephlogisticated air being, after each respiration, purified again by the lime-water, will remain good about thirty times as long as it would when breathed in the ordinary way; and
that

that thus the quantity of dephlogistified air necessary for one minute will now serve for breathing during half an hour, and thus the expences will be thirty times less.

We consume, in the space of 24 hours, about 21600 cubic-inches of air. One pound of nitre yields by heat about 12000 cubic-inches of dephlogistified air, and thus it yields more air than any patient could consume by breathing this beneficial air the whole day (for we must allow at least 12 hours in the 24 for sleeping and necessary occupations), because this quantity will serve thirty times longer by the method explained, than in the ordinary way. It follows by this, that the expences required for breathing a whole day dephlogistified

cated air cannot amount to one shilling.

I have only just time enough to announce this happy discovery to the publick; whose great utility will, I trust, soon be found in the curing of inflammatory and putrid diseases, &c. in which too great a quantity of inflammable principle is let loose in our blood.

I HAVE

I HAVE also discovered, since my book was printed, that, in reading Dr. Priestley's last work, I had overlooked a remarkable passage, p. 270, in which he hints at what I found to be the case with inflammable air having stood a long while with plants. I think it my duty to relate his own words: "I am
 " satisfied, however, from my own
 " observations, that air may be
 " very offensive to the nostrils, pro-
 " bably hurtful to the lungs, and
 " perhaps also in consequence of
 " the presence of phlogistic matter
 " in it, without the phlogiston be-
 " ing so far *incorporated with it*,
 " as to be discovered by the mix-
 " ture of nitrous air."

E R R A T A, necessary to be corrected.

- Page 35, l. 20, before the words *we may find* add *if we could trace the final causes of all the operations of nature,*
- Page 43, l. 7, for *of which quantity gathered* read *which quantity was gathered*
- Page 67, l. 5, for *leaving* read *living*
- Page 97, l. 11, after *nitrous* add *and common*
- Page 120, l. 5, read *which time may discover how to separate, and thus*
- Page 142, l. 10, *and this atmosphere* read *and the air*
- Page 187, l. 2, read *by Abbé Fontana's method, because only one measure of dephlogisticated air was employed.*
- Page 290, l. 6, read *of bubbles when it was heated in the sun, was so much recovered, that fresh leaves*

Explication of some Technical Terms.

AS this volume may possibly fall into the hands of some who have not yet read the works of Dr. Priestley, and are entirely strangers to the new doctrine of air; I think it will be useful to explain the meaning of the new invented names given to different kinds of air mentioned in this book.

Nitrous air is that kind of *permanent elastic fluid* which is extracted by nitrous acid or *aqua fortis* from the most part of metals, principally from mercury, brass, copper, &c. This air is by a bended glass tube conducted under

an inverted jar full of water. The nitrous air, thus disengaged, rises through the water, and settles at the inverted bottom of the jar. Mercury yields the strongest nitrous air, and always of the same quality; but heat must be applied for its extrication, if a greater quantity is in a short time required. I use for convenience fake copper, from which nitrous air is extracted in abundance in a short time, without applying heat. The nitrous acid must be diluted with water for the purpose.

Inflammable air is that air which rises up in bubbles from stagnant waters, whose bottoms are marshy, when the ground is stirred up with a stick. It is also extracted from iron, zinc, and some other metals, by diluted vitriolic or marine acid.

This

This air has in common with almost all other inflammable substances, that it is not susceptible of actual inflammation, without it be in contact with common or respirable air. This air has the following qualities by which it may be known: it is not absorbed by shaking it in water; it is not diminished by the addition of nitrous air; it is instantly and absolutely mortal to animals breathing in it; it burns by the approach of the flame of a candle, where it is in contact with common air; but the whole of it inflames at once, and explodes with a loud report, when it is intimately mixed with common, and principally with dephlogisticated air.

Phlogisticated air is, properly, air impregnated with phlogiston, or with the inflammable principle. It

has received this name because common air, exposed to the calcination of metals, becomes phlogisticated air; which alteration seems to depend on the phlogiston of the metal being communicated to it, for the metal itself has lost it in the calcination; and because the burning of a candle, and many other phlogistic processes, impart to common air this quality. The air returning from our lungs is said to be phlogisticated more or less, because it is found to possess the same qualities as the air exposed to the calcination of metals. This kind of air is known by the following properties: it is not absorbed by water; it is not, or not much, diminished by nitrous air; it is poisonous for an animal who breathes in it; it is not inflammable either by it-
 self

self or by the addition of respirable air, but extinguishes flame.

Dephlogisticated air is that pure, etherial, permanent, and elastic fluid which the celebrated Dr. Priestley found out the first, and gave this very well adapted name to it. It is respirable air, destitute of the phlogistic or inflammable principle with which the best atmospheric air is found always to be more or less contaminated. It is in reality common air of the highest purity, such as never exists in the common atmosphere. It is even so far superior in purity to common air, that an animal shut up in a vessel, filled with this air, protracts its life four or five times, nay even in some cases seven times longer, than if it was shut up in a vessel filled with the best atmospheric or common air.

Some

Some of its qualities, by which it may be known, are the following : it is not abforbable by the contact of water; the flame of a candle plunged in it becomes larger, and of the most admirable brilliancy, so as to dazzle the eyes; red-hot charcoal plunged in it becomes shining and sparkling; it is much more diminished by nitrous air than common air; it explodes, with an uncommon loud report, when mixed with a certain proportion of inflammable air, or when a few drops of good vitriolic æther are poured in a vessel containing this air, as I discovered.

Fixed air is that kind of aerial fluid which issues in abundance from fermenting substances, and which, in some places, rises out of the ground by itself, as in the famous

mous *Grotta del Cane* near Naples. It is this air with which some mineral waters are impregnated, and to which they owe their pungent taste and their virtue, as, for instance, Seltzer waters. It is that air which arises in abundance from calcarious stones, by the addition of vitriolic acid. This air may be known by the following properties: it extinguishes flame; it is absorbed by water, and communicates to it the same pungent, acidulous taste as Seltzer water has, so as not to be distinguished from it either by the taste or by its virtues; it precipitates quick lime from water; it immediately crystallizes *oleum tartari per deliquium*, when put in a vessel lined with this oil; it is mortal to animals breathing in it.

Eudio-

Eudiometer, is a new word; it signifies an instrument by which we may judge of the degree of salubrity of the common air. The invention of such an instrument belongs to Dr. Priestley. It consists chiefly of a glass tube, divided in equal parts; for instance, in two large divisions; each of which is divided into ten others, and each of these ten sub-divided again into ten parts: and a glass measure, containing exactly one of the great divisions of the tube. One measure of common air and one of nitrous air, put together in a separate glass vessel, and left by themselves till the diminution of the bulk of the two airs is compleated, and afterwards let up in the glass tube, indicates at once the exact diminution of the two joint measures. The degree of
 6 goodnefs

goodness of the common air is found to be in proportion to the diminution of the bulk of the two airs. Mr. Magellan, F. R. S. has published a work upon an ingenious contrivance of his own of this kind, which is to be sold by Mr. Parker in Fleet-Street, with the proper directions how to use it. What considerable improvements we owe in this regard to Abbé Fontana, will appear in the introduction to the second part of this work.

ADVER-

ADVERTISEMENT.

AS the author intends to publish a French translation of this work, he thinks it his duty to give public notice of his intentions, that no one may give himself any unnecessary pains about it,

CON-

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ON THE
N A T U R E
O F
P L A N T S.

SECTION I.

*Some general remarks on the nature
of the leaves of plants, and their
use.*

IT seems to be more than probable, that the leaves, with which the most part of plants are furnished during the summer in temperate climates, and perpetually in
B hot

hot countries, are destined to more than one purpose. Such a great apparatus, which nature displays as soon as the sun begins to afford a certain degree of warmth upon the surface of the earth, can scarcely be considered as solely destined either to ornament, to nourishment of the plant, to its growth, to ripen its fruit, or for any other peculiar and single use. It seems probable, that they are useful to the growth of the tree; for, by depriving the tree of all its leaves, it is in danger of decay. By taking a considerable part of the leaves from a fruit tree, the fruit is less perfect; and by taking them all away, the fruit decays and falls before its maturity. It is also probable, that the tree receives some advantage from the leaves absorbing, by their means, moisture.

moisture from the air, from rain, and from dew; for it has been found a considerable advantage to the growth of a tree, to water the stem and the leaves now and then. But I leave the discussion of those articles to others, who have made these considerations an object of their pursuits. The late Mr. Baker has published to the world his microscopical observations on the subject. Mr. Bonnet, of Geneva, has published a very elaborate work upon the same, intitled, *Recherches sur l'usage des Feuilles dans les Plantes, et sur quelques autres Sujets relatif à l'Histoire de la Vegetation, par Charles Bonnet, à Gottingen et Leiden, 1754.* This work contains a great deal of interesting inquiries upon the nature, properties, and utilities of those wonderful organs; all which

have been treated with the greatest attention, and have thrown much light upon this subject.

This celebrated author has taken a great deal of notice of those air bubbles which cover the leaves when plunged under water. He says, p. 26, that the leaves draw these bubbles from the water. He is the more persuaded that this is the case, because he found these bubbles did not appear when the water had been boiled some time, and appeared more when the water is impregnated with air, by blowing in it. He had also observed, that they did not appear after sunset. Page 31, he explains his opinion farther upon this head: he says, that these air bubbles are produced by common air adhering to the external surface of the leaves,

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which

which fwells up into bubbles by the heat of the fun; and that the cold of the night is the reason why these air bubbles do not make their appearance at that time. As he found that dry leaves put under water gather such bubbles also upon their surface, he concludes, p. 33, that the appearance of these bubbles cannot be owing to any vital action in the leaves.

I took some pains to disclose the cause of these bubbles, which, I think, are of more importance than Mr. Bonnet at that time imagined them to be; and found the fact to be generally this:

The most part of leaves gather these bubbles upon their surface, when plunged in any water in the sun-shine or by day-time in the open air; but infinitely more in

fresh pump water than in any other. In clear river water they appear later, less in number and in size; less so in rain water, and the least of all in boiled water, in stagnating, and in distilled water.

They are not produced by the warmth of the sun rarifying the air adhering to the leaves; for many kinds of leaves produce them almost as soon as plunged under water, though the water be very cold, and the leaves warm from the sun-shine be plunged in it.

They do not appear after sun-set, at least not in any considerable number; but those that already exist do not shrink in or disappear by the cold of the night.

As soon as the sun begins to diffuse its warmth over the surface of the earth in the spring, and to promote

mote that general tendency to corruption which all dead bodies of the animal and vegetable kingdom, and many other substances, are so liable to, the trees display in a few days the most wonderful scene that can be imagined. Contracted as they were in that state of stupor and inactivity in which they remain during the winter, exposing to the air no other surface than that of their trunk and branches, as if they wanted to have as little to do as possible with the external air, they all at once increase, perhaps more than a thousand times, their surface by displaying those kind of numberless fans which we call leaves. Some of them produce their leaves a long while before any flowers appear upon them; others a good while after the flowers are formed, and

the fructification is already in an advanced state; and keep their leaves in the best condition, and even push out continually new ones, long after the whole fructification is finished; which seems to indicate, that the chief use of these fans is not to assist the fructification and propagation of their species. These fans, when compleated, seem to compose or arrange themselves in such a manner as to expose their upper and varnished surface to the direct influence of the sun, and to hide as much as they can their under surface from the direct influence of this luminary. It seems as if they required rather the light of the sun than the influence of its heat, as their polished surfaces must reflect some of the rays of the sun, and thus moderate the degree of heat.

It

It will, perhaps, appear probable, that one of the great laboratories of nature for cleansing and purifying the air of our atmosphere is placed in the substance of the leaves, and put in action by the influence of the light; and that the air thus purified, but in this state grown useless or noxious to the plant, is thrown out for the greatest part by the excretory ducts, placed chiefly, at least in far the most part of plants, on the under side of the leaf.

Is there not some probability that the under part of the leaves may have been chiefly destined for this purpose; because in this way the dephlogisticated air, gushing continually out of this surface, is inclined to fall rather downwards, as a beneficial shower for the use of the animals who all breathe in a region of
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the air inferior to the leaves of trees? Does not this conjecture get some weight, if we consider that dephlogisticated air is in reality specifically heavier than common air, and thus tends rather to fall downwards?

If we add to these reflexions another of no less importance, *viz.* that most sorts of foul air are specifically lighter than common air, we shall be inclined to believe that the difference of the specifical gravity of that beneficial air of which I treat, and that which is become hurtful to our constitution by corruption, breathing, and other causes, indicates one of those special blessings designed by the hand of God: for by this arrangement we get soon rid, in a great measure, of that air which is become hurtful to us, as it rises soon up out of our reach; whereas
the

the dephlogifticated air, being heavier than common air, is rather inclined to settle on the surface of the earth among the animal creation.

But, as animals spoil equally as much air in the winter as in the summer by the act of respiration, it might seem somewhat surprizing, that this great laboratory ceases entirely by the decay of the leaves. Is this defect supplied by some other means equally powerful? Though we are very far from being able to trace all the active causes which contribute their share in keeping up the wholesomeness of our atmosphere, yet we have already traced some of them, and therefore must not despair of discovering some more. The shaking of foul air in water will in great measure correct it. Water itself has a power
of

of yielding dephlogisticated air, as Dr. Priestley discovered. Plants have a power to correct bad air, and to improve good air. Winds will blow away the noxious particles of the air, and bring on air corrected by the waters of the seas, lakes, rivers, and forests. All these causes exist equally in the winter as in the summer, or at least nearly so. The influence of the vegetable creation alone ceases in the winter: but the loss of this influence is, perhaps, more than amply counterbalanced by the diminution of the general promoting cause of corruption, *viz.* heat. Every body knows, that warm weather hastens in a great degree putrefaction. In the summer time numberless insects are produced, which did not exist in the winter: these insects infect the air by the corruption of their

their bodies. That immense quantity of animal substances, and many others, which undergo a putrefaction by the warmth of the weather, seems to require an additional power or agent to counter-act it; and this office is destined to the leaves. In frosty weather no animal substance is subject to putrefaction, which cannot go on without a proper degree of heat. The perspiration of animals is less offensive in the winter than in the summer, and of consequence must corrupt the atmosphere less. It seems therefore probable, that, if we are deprived of one way by which air is corrected in the winter, we have also at that time less causes which tend to contaminate our element.

SECTION II.

On the manner in which the dephlogisticated air is obtained from the leaves of plants.

AS the leaves of plants yield dephlogisticated air only in the clear day-light, or in the sun-shine, and begin their operation only after they have been in a certain manner prepared, by the influence of the same light, for beginning it; they are to be put in a very transparent glass vessel, or jar, filled with fresh pump water (which seems the most adapted to promote this operation of the leaves, or at least not to obstruct it); which, being inverted in a tub full of the same water, is to be immediately exposed to the open air, or rather

rather to the sun-shine: thus the leaves continuing to live, continue also to perform the office they performed out of the water, as far as the water does not obstruct it. The water prevents only new atmospheric air being absorbed by the leaves, but does not prevent that air, which already existed in the leaves, from oozing out. This air, prepared in the leaves by the influence of the light of the sun, appears soon upon the surface of the leaves in different forms, most generally in the form of round bubbles, which, increasing gradually in size, and detaching themselves from the leaves, rise up and settle at the inverted bottom of the jar: they are succeeded by new bubbles, till the leaves, not being in the way of supplying themselves with new atmospheric

spheric air, become exhausted. This air, gathered in this manner, is really dephlogisticated air, of a more or less good quality, according to the nature of the plant from which the leaves are taken, and the clearness of the day-light to which they were exposed.

It is not very rare to see these bubbles so quickly succeeding one another, that they rise from the same spot almost in a continual stream: I saw this more than once, principally in the *nymphaea alba*.

SECTION III.

The manner in which the dephlogisticated air oozes out of the leaves is different in different plants.

IT is somewhat amusing to observe the constant manner in which the dephlogisticated air makes its appearance upon the same kind of leaves, and the surprizing difference which takes place in the leaves of different plants. Some leaves, for instance, form always small round bubbles, as is the case with the most part of leaves; others form irregular flat blisters, as do the leaves of the honey-suckle, *caprifolium*. Some, and indeed the greatest part, produce round bubbles on both surfaces; others yield on one surface round bubbles,

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on

on the other irregular blisters; for instance, leaves of oak, which give flat blisters on the under side, and round bubbles on the upper side. Some form only those irregular blisters at the upper side, as the leaves of spurge, *cataputia* or *euphorbia*.

Some leaves form neither bubbles nor blisters on either side, and yet yield a great deal of dephlogisticated air; for instance, leaves of *nasturtium Indicum*: these leaves seem to have a quality repulsive to water, which only forms a kind of cover over the surface of the leaves, without coming into immediate contact. The air oozing out of the leaves floats under this cover, and rises to the highest part of the leaves, where it forms a kind of bags, which at last detach themselves from the edge, and rise to the top of the jar. The leaves

leaves themselves, after standing a day and a half in the water, are not wetted by it, but come out quite dry.

Some leaves have that peculiar quality of being wetted by the water only on one side; as, for instance, leaves of raspberry shrubs, which do not receive the wet on their shaggy under surface.

Strawberry leaves repel the water from both surfaces, form blisters at the under surface, and chiefly round bubbles at the upper surface.

Some leaves begin very early in the morning to yield dephlogisticated air, and cease late in the evening; for instance, potatoe and *malva* leaves.

Some begin this operation very late in the morning, and cease very early

early in the evening; for instance, leaves of *laurocerasus*.

Some leaves yield the air bubbles immediately, as leaves of potatoe plants; some in a few seconds, as *malva*; some in a few minutes, as walnut trees; some much later, as leaves of *laurocerasus*.

Some yield the air bubbles first on the under side, as almost all leaves of trees; some first at the upper side, as leaves of *laurocerasus*; some on both surfaces at the same time, as *malva*.

On some leaves the air bubbles grow almost all regularly one with another, as in leaves of a vine, walnut, lime tree; in some they are, from the very beginning, of a very irregular size, as in *malva*, parsley, &c.

These

These few instances shew the various ways in which this beneficial air is oozing from the leaves, and which can only be owing to the different organization of the leaves in different plants.

I have discovered a good deal more of such remarkable peculiarities among leaves of different plants; but those above mentioned will be sufficient to shew, that each plant follows in this regard its own nature; and that, therefore, these different appearances seem to depend upon some vital motion going on in the substance of the leaves.

SECTION IV.

The dephlogisticated air oozing out of the leaves in the water is not air from the water itself.

THE reverend Dr. Priestley found, that water, chiefly pump water, standing some days by itself, forms at the bottom and sides of the vessel a kind of green matter, seemingly vegetable, from which air bubbles rise continually to the top of the jar, if exposed to the sun-shine: that this air is fine dephlogisticated air, which shews that there is a faculty in water to produce by itself this beneficial fluid; and thus, that the mass of the waters of the sea, lakes, and rivers, have their share in purifying the atmosphere.

But

But as this dephlogisticated air is not produced immediately from the pump water, but only when this green matter is formed, it is clear, that the air obtained from the leaves, as soon as they are put in the water, is by no means air from the water, but air continuing to be produced by a special operation carried on in a living leaf exposed to the day-light, and forming bubbles, because the surrounding water prevents this air from being diffused through the atmosphere.

It is true, that pump water, placed in the sunshine, will soon yield some small air bubbles, settling at the bottom of the jar, and every where at the sides; but this air is very far from being the same as that contained in the air bubbles of the leaves.

I placed, in a warm sun-shine, a great number of inverted jars, full of pump water, and collected carefully from them all the air yielded by these bubbles, which proved to be much worse than the common air.

I boiled some pump water in a pot, in which I had placed a long cylindrical jar, quite full of the same water: a good deal of air was collected at the top of the inverted jar, which was by the heat disengaged from the water. This air proved to be much worse than common air, and entirely unfit for respiration.

Abbé Fontana has made, some years ago, a great many experiments, tending to investigate the nature of air contained in different waters.

SECTION V.

The dephlogisticated air oozing out of the leaves in the water is not existing in the substance of the leaves in this pure state, but is only secreted out of the leaves when it has undergone a purification, or a kind of transmutation.

IF the dephlogisticated air collected from the leaves in the sun existed in them in its pure state, it must appear as such when squeezed out of the leaves under water; or, at least, if the leaves are only shook gently under water, without hurting their organization, or when they are put in warm or in boiling water.

I squeezed a handful or two of potatoe leaves under water, and kept
an

an inverted jar full of water above it, to receive the air. A great deal of it was instantly obtained, which proved to be nearly as good as common air.

I squeezed, in the same way, some air out of leaves of sage, *salvia*, which proved to be somewhat worse than the former.

A potatoe plant was shook under water, so as not to hurt it: a good deal of air was immediately disengaged, which, by the nitrous test, proved to be worse than common air.

A plant of *lamium album* was treated in the same way, and in like manner a good deal of air was obtained, which was nearly of the same quality with the former.

Some leaves of an apple tree were put in a cylindrical jar full of pump water. The jar was then inverted
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in a vessel full of the same water, and placed upon the fire. As soon as the water grew warm, the leaves were covered with air bubbles, just as in the sun. After the water had boiled a little while, it was put by to cool. A great deal of air was obtained, which proved to be so bad as to extinguish flame.

Some of the same leaves were put into a jar, inverted in a pot full of water, and only placed near the fire: a great deal of air was obtained, but as poisonous as the former.

SECTION VI.

The production of the dephlogisticated air from the leaves is not owing to the warmth of the sun, but chiefly, if not only, to the light.

IF the sun caused this air to ooze out of the leaves by rarifying the air in heating the water, it would follow that, if a leaf, warmed in the middle of the sun-shine upon the tree, was immediately placed in water drawn directly from the pump, and thus being very cold, the air bubbles would not appear till, at least, some degree of warmth was communicated to the water; but quite the contrary happens. The leaves taken from trees or plants in the midst of a warm day, and plunged

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immediately into cold water, are remarkably quick in forming air bubbles, and yielding the best dephlogisticated air.

If it was the warmth of the sun, and not its light, that produced this operation, it would follow, that, by warming the water near the fire about as much as it would have been in the sun, this very air would be produced; but this is far from being the case.

I placed some leaves in pump water, inverted the jar, and kept it as near the fire as was required to receive a moderate warmth, near as much as a similar jar, filled with leaves of the same plant, and placed in the open air, at the same time received from the sun. The result was, that the air obtained by the fire

fire was very bad, and that obtained in the sun was dephlogisticated air.

A jar full of walnut tree leaves was placed under the shade of other plants, and near a wall, so that no rays of the sun could reach it. It stood there the whole day, so that the water in the jar had received there about the same degree of warmth as the surrounding air (the thermometer being then at 76°); the air obtained was worse than common air, whereas the air obtained from other jars kept in the sunshine during such a little time that the water had by no means received a degree of warmth approaching that of the atmosphere, was fine dephlogisticated air.

No dephlogisticated air is obtained in a warm room, if the sun does not shine upon the jar containing the leaves.

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SECTION VII.

Reflections.

IT might, perhaps, be objected, that the leaves of the plants are never in a natural state when surrounded by pump water; and that thus there may, perhaps, remain some degree of doubt, whether the same operation of the leaves in their natural situation takes place.

I cannot consider the plants kept thus under water to be in a situation so contrary to their nature as to derange their usual operation. Water, even more than they want, is not hurtful to plants, if it is not applied too considerable a time. The water only cuts off the communication with the external air; and we know,
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that plants may live a long while without this free communication. Besides, water plants, as *persicaria urens*, *becabunga*, and others, which I have employed in my experiments, are often found a long while quite covered by the water in which they grow.

By bending a living plant (the root remaining in its own earth) in an inverted jar full of water, you only surprize nature upon the fact in the middle of its operation, by shutting at once all communication with the free air. In such a situation no air can be absorbed by the leaves, or by any parts of the plant under water; but any air may freely come out of it.

Without covering the leaves or the plant entirely with water, it is impossible to know what quantity
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of air oozes out of the plant, and of what quality this air is; for any air issuing out of a plant incorporates immediately with the surrounding air, and makes a compound whose constituent parts are an intimate mixture of air from the plant and common air; and it would be as difficult to judge accurately how much dephlogisticated air such a plant has communicated to the ordinary air which was already in the jar, as it would be for a chymist to judge accurately what quantity of distilled water was mixed with a certain quantity of common water, if some of it was really added to it on purpose to puzzle him. It may, however, be ascertained, in an inaccurate way, what quantity of this beneficial air a plant, placed in a jar full of common air, has communi-

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cated to it, by computing the degree of superior goodness the air is found to possess.

As plants yield in a few hours such a considerable quantity of dephlogistified air, though their situation seems rather unfavourable for it when they are kept under water ; may it not with some degree of probability be conjectured, that they yield much more of it when remaining in their natural situation ; for then, being continually supplied by new common air, their stock of dephlogistified air cannot be exhausted. It is an unfavourable circumstance, that air is not an object of our sight ; if it was, we should perhaps see that plants have a kind of respiration as animals have ; that leaves are the organs of it ; that, perhaps, they have pores

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which

which absorb air, and others which throw it out by way of excretion, as are the excretory ducts of animals; that the air secreted, being dephlogisticated air, is thrown out as noxious to the plant (which article is clearly demonstrated by Dr. Priestley and Mr. Sheele); that in the most part of plants, principally trees, the greatest part of inhaling pores are placed upon the upper side of the leaf, and the excretory ducts principally on the under side.

If these conjectures were well grounded, it would throw a great deal of new light upon the arrangement of the different parts of the globe, and the harmony between all its parts would become more conspicuous. We might find, that partial tempests and hurricanes, by shaking the air and the waters, pro-

duce some partial evils for the universal benefit of nature; that, by these powerful agitations, the septic and noxious particles of the air are blown away, and rendered of no effect, by being thus diluted with the body of air, and partly buried in the waters. We might conceive a little more of the deep designs of the Supreme Wisdom in the different arrangement of sublunary beings. The stubborn atheist would, perhaps, find reason to humiliate himself before that Almighty Being, whose existence he denies because his limited senses represent to him nothing but a confused chaos of miseries and disorders in this world.

SECTION VIII.

Dry plants have very little or no power to affect air; but, when moistened, they infect air.

I FILLED a jar with dry hay, another with dry straw, and left it inverted a good while; but could not find the air altered.

I put some leaves of a lime tree, dried for the purpose, in a jar full of fresh pump water; and placed another jar, filled with an equal number of fresh leaves of the same tree, near it in the sun.

The dry leaves began much later than the others to yield round bubbles just in the same way, but which did not grow so quick, nor so large. However, in a few hours, a good

deal of air was obtained, but of such a bad quality as to extinguish a flame; whereas the fresh leaves had yielded dephlogisticated air: which experiment seems to indicate, that the generation of the dephlogisticated air is owing to the action of the living plant. The same result was also obtained from dry hay put into a jar full of pump water.

SECTION IX.

All plants possess a power of correcting, in a few hours, foul air unfit for respiration; but only in clear day light, or in the sun shine.

THIS remarkable property of plants is indeed very great; for in a few

few hours, nay even sometimes in an hour and an half, they purify so much a body of air quite unfit for respiration, as to be equal in goodness to atmospheric air. They will even do it when they are inclosed in a glass vessel, without any water. One leaf of a vine, shut up in an ounce phial, full of air fouled by breathing so that a candle could not burn in it, restored this air to the goodness of common air in the space of an hour and a half. But plants enjoy this privilege only in the day-time, and when they grow in unshaded places.

This power of plants extends itself even to the worst of all airs, in which an animal finds his destruction in a moment; such as is pure inflammable and highly phlogisticated air, which is little or scarcely at

all diminishable by nitrous air. I observed some difference in various kinds of plants in this respect, and found that water plants seem to possess this quality in a greater degree than others. The willow tree and the *persicaria urens* were found eminent in producing this effect: and may it not be providentially ordained it should be so, as those plants grow better in marshy, low grounds, and even in stagnated waters, whose bottoms are generally muddy, and yield a great deal of inflammable air, which may be collected at the surface of the water by stirring up the ground, and may be kindled by throwing a burning paper upon the water, which is an amusing experiment by night? Plants, however, want longer time to correct this kind of air, at least that which is extracted

tracted from metals by vitriolic acid.

The property of plants is demonstrated in experiments 41, 51, 56, 57, 58, 59.

SECTION X.

All plants yield a more or less quantity of dephlogisticated air in the daytime, when growing in the open air, and free from dark shade.

THE quantity of dephlogisticated air, and even the quality of it, which the leaves of plants give, seems to be different in different plants: though, indeed, this may depend in a great measure upon some particular

ticular circumstances, to which it is not easy to be sufficiently attentive. It seems, however, to be a general rule, that the leaves of all plants, growing in a place where they are not much shaded by other plants, buildings, &c. yield, in a clear day, dephlogisticated air; and that this air is yielded in greater abundance, and of a greater purity, when they grow in open places unincumbered by other plants higher than they are themselves.

I got in general a large quantity of air of a very good quality from some water plants, as from the *persicaria urens* and willow. The fir trees yielded also very fine air, and in abundance.

The *nasturtium Indicum* surpassed them all in general, in regard as well to the quantity as to the quality.

lity. One hundred leaves of this plant, which are very thin, yielded, in two hours time, as much dephlogisticated air as would fill a cylindrical glass four inches and a half deep, and one inch and three quarters diameter; of which quantity gathered again afterwards from the same leaves, without taking them out of the water, see exp. 30—35. This quantity surpasses by far the bulk of the leaves themselves, and shews to how amazing a quantity the air may amount yielded in a fair day by a lofty tree.

The leaves being more or less crowded together, being exposed for a longer or shorter time, or sooner or later in the day, will occasion some difference in the quality and quantity of this air.

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It seems that, in general, the finest air is obtained when the sun has passed the meridian.

SECTION XI.

The faculty which plants possess of yielding dephlogisticated air, of correcting foul air, and improving ordinary air, is not owing to the act of vegetation.

IF this wonderful faculty of plants depended upon their vegetation, they would exert it at all times, and in all places in which the vegetation goes on. A plant may vegetate, and even thrive very well, in the utmost darkness; and yet in such a place

place it has no power to correct bad air, or to yield good; but, on the contrary, it spreads round about it deleterious exhalations, which render the best air even pernicious to the utmost degree.

It will not be difficult to understand now from what cause all those different and contrary effects which Dr. Priestley has found in his experiments did really depend; and why Mr. Sheele had constantly found that the vegetation of beans always spoilt good air.

These gentlemen expected the good effects from the vegetation of the plants, as such. By making a plant grow night and day in ordinary air kept in a phial with the plant, the effect will depend upon the greater or less exposure of the plant to the light. Besides, by keep-
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ing a plant a long while in pump water, the green matter, of which Dr. Priestley found to issue very fine dephlogisticated air, will be generated; and thus the air within the phial, being mixed with this good air, will not in reality indicate the effect of the plant upon this air, as Dr. Priestley makes no scruple to acknowledge in his late work, p. 338.

SECTION XII.

The plants evaporate by night bad air, and foul the common air which surrounds them; yet this is far over-balanced by their beneficial operation in the day.

THE bad air which plants yield by night is so inconsiderable in comparison of the quantity of dephlogisticated air which they yield by the day-time, that it amounts to very little. By a rough calculation I found, that the poisonous air yielded during the whole night by any plant could not amount to one-hundredth part of the dephlogisticated air which the same plant yielded in two hours time in a fair day. But, from my experiments,

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one might naturally wonder that no remarkable mischief happens from so many plants as a forest contains, if one plant, containing scarce a handful, may poison to such a degree the quantity of two pints of common air in one night, as to render it absolutely mortal for any animal who breathes in it.

I make no doubt but a great quantity of plants, kept in a close and small place during a night, or by day in the dark, may do some material mischief, and even occasion death, to any person who should be imprudent enough to remain in such a place. The undoubted facts of people being found dead in their beds, when they had slept in a small room with a great deal of flowers in it, must inspire us with a caution against keeping too many flowers in
close

close rooms. My experiments go much further, and will, I hope, in future, make people aware of danger if they store up a great deal of fruit in a close room in which they sleep.

I think that nobody before me even suspected the least danger of keeping beans, peaches, or such like fruits, in their rooms; and yet a sufficient number of them would easily poison an unwary person sleeping in a close room, in which an abundance of these fruits is stored.

The gardeners by opening a hot-house early in the morning, which has been shut close during the night, or at any time in the day if the sun has not shined a good deal upon it, are very well aware of a particular oppression they feel by entering it. I remember to have felt it more

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than

than once, without even suspecting the cause of it. Dr. Priestley observed this remarkable offensiveness of the hot-houses with a more philosophical attention; he tried the air within them, and found it worse than common air.

By all this it seems evident, that it would not be safe to sleep in a close hot-house; that it would not be prudent to keep too many green branches, fruit, or flowers, in any room by night, particularly in that of a sick person.

The best physicians have, indeed, often recommended to put green branches of lime-trees and others in the rooms of their patients, without ever suspecting any other effect but benefit from them. I think still, that some benefit may arise from putting, in a clear day, fresh green branches

branches in the room of a sick person, by cooling the air, which is owing to the evaporation of moisture; but I should now apprehend rather some mischief from them in a room whose doors and windows are shut, and which is not well lighted. At any rate, I should no more allow them to be kept in the night-time in the room of any of my own patients.

Is it not somewhat probable, that among those people who are found dead in their bed without any previous illness, some may owe their untimely end to some such concealed cause, which nobody ever suspected to be in any way dangerous?

But the mischief which trees in reality do by night-time to the surrounding air, cannot do any observable harm to animals: for

those mischievous exhalations being, very providentially, specifically lighter than common air, rise at the same time up; and thus the lower region, in which we breathe, is freed from them almost as soon as they are produced; whereas the dephlogisticated air issuing out of the plants in great abundance in the day-time is specifically heavier than common air, and is therefore inclined to remain longer among us, and to afford us all the benefit for which the Supreme Wisdom has providentially destined it.

SECTION XIII.

All roots, few excepted, when left out of the ground, yield by day and by night foul air, and infect the surrounding air.

THE experiments I made upon this subject convinced me that roots have this deleterious power, and some even to so great a degree, that it would not be safe to remain in a small close room in which a great quantity of roots of plants are kept. The roots of some aquatic plants are remarkably apt to foul the air in which they are placed, such as roots of rushes, though ever so well cleaned from mud and dirt, and the roots of *persicaria urens*. But I found the roots of *becabunga*

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almost

almost inoffensive in this respect, which I was the less surprized at, as their substance is but little different from the stalks. The roots of a mustard plant gave in the sun-shine a good deal of air, when kept under water; which air was worse than common air, and extinguished a candle. These roots even corrupted good air in the middle of the sun-shine.

A plant, with roots and all, placed in a jar full of water, did, however, yield dephlogisticated air; so that the bad effect of the roots was overbalanced by the plant itself.

SECTION XIV.

Flowers ooze out by day and night an unwholesome air, and spoil, at any time and in every place, a considerable body of air in which they are placed.

DR. Priestley has observed, that a rose, kept in a glass, had, in a short time, so much infected the air as to be unfit for respiration, and concluded from this very justly, that flowers might be hurtful in a room. I have heard more than once of a person found dead in a room where a great deal of flowers were kept; and I make no doubt but some of these cases are well founded.

I tried all the flowers I could find in my neighbourhood, but could

not discover one which did not yield poisonous air, though in a small quantity, by day and by night, and which had not the power of rendering quite unfit for respiration a very considerable body of common air. They even seem not to lose in the least their deleterious influence in the sun; so that I cannot but think that it is unsafe to keep in a close room a large quantity of any flowers, even such as have the most delightful smell. I am, however, very far from thinking that there is any danger to apprehend from such nosegays as are commonly kept in a room, either for ornament or perfume. The malignant influence which could be expected from such a small quantity of flowers is entirely dissipated in the mass of the surrounding air; but the excess must

must not go too far, if the room is closely shut and but small. If a few flowers of the honey suckle (*caprifolium*), which possess the most agreeable smell, are able to foul in three hours time, in the middle of the day, a body of air equal to two pints (see experiments 65—70) we may judge what dangerous effect might be expected from a large quantity in a close room. Those flowers, like all others, after having rendered truly fatal a body of air, have lost nothing of their flavour. The air itself, which they have poisoned, is impregnated with the same fragrant smell as the flowers themselves; so that a person, not aware of the concealed poison which flowers spread round about them, might be easily induced by the sweetness of their scent

scient to run the greatest hazard of losing his life, without the smallest apprehension of danger.

SECTION XV.

All fruits in general exhale a deleterious air by day and by night, in the light and in the dark, and possess a remarkable power of spreading a poisonous quality through the surrounding air.

I WAS, indeed, not a little surprized to find this effect in even the most delicious fruit, such as peaches, grapes, apples, and mulberries. By what I observed in my experiments I am apt to think, that the power of fruit,

fruit, at least of some, surpasses the deleterious quality of flowers in the dark; but the influence of the sun seems to check, in some degree, this hurtful quality in some fruit more than in roots and flowers, of which the most part preserve, even in the brightest sun-shine, their virulent effects upon the ambient air in its full force. I found, that one peach was able to render intirely poisonous, in a few hours, a body of air at least six times greater than the space it occupied; and even that they could, in the middle of the sun-shine, render such a quantity of air so unwholesome, that a candle could not burn, nor an animal breathe in it.

After I had observed, that all leaves of plants yield dephlogisticated air by day-light; and considering, that in general all leaves are
green,

green, and that that substance which Dr. Priestley discovered to yield so much dephlogisticated air is of the same colour; I had some suspicion, that green fruits, such as beans, would also yield dephlogisticated air. I placed, for this purpose, some French beans in a jar full of pump water, and exposed it to a bright sun-shine during four hours, and obtained a moderate quantity of air, issuing out of their substance by bubbles, in the same manner as out of leaves. This air was far from being dephlogisticated air; for it was even worse than common air, though it approached pretty near it in goodness.

I then wanted to see what effect
these green fruits would have upon
a body of air in the dark; and I
was not a little surprized to find,
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that they had a very remarkable power to foul a great body of air, in which they were shut up, to such a degree, that two dozen of small French beans, placed in a jar holding two pints, had rendered in one night the air in the jar absolutely poisonous, so that a young chicken placed in it was killed in less than twenty seconds. I found even this deleterious influence of beans upon air to surpass the power of plants, which are known to be of a poisonous quality. See experiments 75—91, and principally experiments 88 and 89.

Ripe mulberries, filling one-third of a jar, and placed in the sun during four hours, had so much fouled the air within the jar that a candle would not burn in it.

SECTION XVI.

The power of plants in correcting bad air is greater than their faculty of improving good air.

THE experiments already known of Dr. Priestley, by which it appears that plants thrive wonderfully well in air fouled by breathing and burning of candles, gave me a great suspicion, that the power of plants in correcting bad air might surpass their faculty of improving good common air. In order to put my conjecture to the trial, I placed at eleven o'clock, in a warm sun-shine, two jars of an equal size, each containing an equal quantity of sprigs of pepper-mint, in pump water. In one of these jars was let up a certain quantity

tity of common air, whose test was at that time such, that one measure of it with one of nitrous air occupied $1.06\frac{1}{2}$. In the other jar was let up the same quantity of air fouled by respiration, of which one measure with one measure of nitrous air occupied 1.34.

The air of both jars was examined at two o'clock, when I found the common air so much improved, that one measure of it with one of nitrous air occupied 1.00. The foul air was so much mended, that it was near as good as the atmospheric air, for one measure of it with one of nitrous air occupied now $1.08\frac{1}{2}$.

I examined both airs again at four o'clock, when the common air was still more improved; for one measure of it with one of nitrous
air

air occupied 0.95. The foul air now was not only become as good as respirable air, or air of the atmosphere, but even surpassed it in goodness, for one measure of it with one of nitrous air occupied 1.05.

Now, as the same plant brought the foul air from 1.34 to 1.05, and the common air from $1.06\frac{1}{2}$ to 0.95, it appears clear, that the plant had corrected the foul air far more than it had improved the common air.

This experiment was repeated several times with nearly the same results.

As plants seem to delight in foul air, probably because this air impregnated with phlogiston affords more proper nourishment, *viz.* phlogiston to the plant; it must of course happen, that a plant draws to it so much the more phlogiston
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as the air, in which it grows, contains more of this principle.

When a plant grows in the open air, it contaminates by night the surrounding air; but this air, being diluted with other air, does not appear in reality to be altered by any method yet found out: besides, it is probable, that this air is risen up as soon as it was become phlogisticated by the plant, being specifically lighter than common air. It seems therefore not improbable, that some plants, as for instance the *hyoscyamus*, may contaminate in reality more air at night than they improve in the day; so that, if all the air spoiled by such a plant was shut up with the plant a whole night and a day, the air would still be found contaminated: but tho' this might be the case when the plant is shut up with the air,

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yet it could never be any real disadvantage in the natural situation of things, because this fouled air may be corrected in the atmosphere by some manner or other unknown to us ; and, if not, we are, at any rate, immediately out of its reach, as it rises by its being become lighter. But if such infectious plants are shut up in small close rooms, they certainly might do a material injury to our constitution, and even occasion death.

It appears, by experiment 41, that a plant may really foul so much air at night as scarce to be able to correct in the day. But it is to be considered, that such a plant, being maimed by its roots being taken off, and by being shut up in a narrow space, must have lost some of that vigour which plants naturally have when

when they remain undisturbed upon their place. See also experiments 51, 56, 57, 58, 59, 60.

SECTION XVII.

On the effect of leaving plants kept in a room.

THO' I think, that the keeping of a few plants in rooms is very indifferent as to the health of the persons who live in them; yet it is not so indifferent for us to know the effects which plants have in reality on the air of the room, that we may avoid danger from any excess.

The influence of plants on the air of a room in which they are kept is different in the night from what it is in the day. In the day plants are apt to contribute somewhat to purify the air of the room, if they are placed so as to receive all the light of the sun possible : if they are placed so as not to receive the direct influence of the sun, but to be free from any shade, they seem to have no influence at all, either in improving the air of the room or in fouling it. But when they are placed in a part of the room the most remote from the windows, so as to be much shaded, they are apt to render the air of the room more or less impure, in proportion to their size, and to the more or less degree of light of the place where they stand. At night
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they absolutely tend to foul the air, principally when they flower. I acknowledge readily, that a few flower-pots can do neither good nor harm. But I remember to have found several orange-trees in a room, by way of ornament, and, as I was told, to keep the air of the room wholesome : I think now such ornamental plants by no means indifferent, unless they were but small and the room ample ; at any rate I should not suffer them to be kept in a room at night, where a sick person is.

A plant shut up in a glass jar, and placed near the window in a room so as to receive the rays of the sun, will make the air of the jar better than the air of the room : whereas a similar plant, placed in the same room in a shaded place,

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will

will render the air of the jar worse than the air of the room. If, after a few hours, you invert the experiment, by placing the plant which stood at the window in the shade, and that which stood in the shade near the window, the reverse will take place, *viz.* the air of the jar, which was improved, will be found worse than the air of the room; and the air of the jar, which had been contaminated, will be found corrected again. And this remarkable property of plants, in the way just mentioned, may be demonstrated in a few hours. See experiment 45.

SECTION XVIII.

Leaves of plants die sooner when the dephlogisticated air, elaborated by them, is separated from them.

WHEN the dephlogisticated air, settled in the form of bubbles upon the leaves, is shook off, new bubbles succeed; and thus by shaking off several times these air bubbles a greater quantity of dephlogisticated air is obtained. The second crop of bubbles contains in general a finer kind of air than the first; the reason of which may be, that it is scarce possible to free the surface of the leaves entirely from all atmospheric air sticking to them, particularly those which have a rough

shaggy surface ; as, for instance, the leaves of sage, *salvia*.

Some of these leaves are so prolific in pushing out these bubbles, that I have found them reproduced nine or ten times in leaves of a pear-tree. The leaves of a vine are also very ready to yield a good number of successions of these bubbles. But I was curious to see whether the leaves decay sooner or later when the air bubbles were left upon them, or when they were shook off now and then : I put a leaf of a vine in a jar full of pump-water, and left it exposed to the open air without ever stirring it : the air bubbles grew to a very large size ; and some of them quitted the leaf of themselves and rose up. This leaf remained as fresh as when it was put in the jar during a whole week ;

week ; whereas another leaf of the same vine, placed near it in another jar, and whose air bubbles were shook off five or six times in a day, was withered in less than two days. This second leaf had lost the greatest part of the rough surface, which covers, as a kind of scarf-skin, the under and unvarnished part of the leaf ; at least this scarf-skin became transparent, if it was not really destroyed ; and this transparency was observed principally upon the very spots of the air bubbles. This experiment was repeated several times with the same success.

It should seem by this observation, that the loss of this air, if it cannot be replaced by the absorption of new air from the atmosphere, makes the leaves decay sooner ; and thus the texture of
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the leaves, having no more air to elaborate, resembles almost the organization of an animal, which loses its life by becoming exhausted through the losses sustained by the increase of the various excretions which are carried on in its body, if these losses are not repaired by taking in new nourishment.

Vegetables seem to draw the most part of their juices from the earth, by their spreading roots ; and their phlogistic matter chiefly from the atmosphere, from which they absorb the air as it exists. They elaborate this air in the substance of their leaves, separating from it what is wanted for their own nourishment, *viz.* the phlogiston, and throwing out the remainder, thus deprived of its inflammable principle, as an excrementitious fluid, and in this
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state hurtful to them, but rendered useful to the animals, who in their tour take from this air, by the act of respiration, what they want, and throw out the remainder as hurtful to them; but rendered again serviceable to the vegetables. This theory seems to be very reasonable, and to have some foundation in nature. It throws a good deal of light upon the œconomy of nature, and the mutual influence which the vegetable kingdom has upon the animal, and the animal upon the vegetable. It has some analogy with other general operations of nature, which are well known.

A plant, which is a living being, destitute of motion, remaining upon the same spot on which it took its beginning, if not capable, as animals are, of going in search of its food,
must

must find within the narrow compass of the space it occupies every thing which is wanted for itself, and to fulfil the office which has been dictated to it by the Author of nature. It is obliged to spread the numberless filaments of its root through the surrounding ground, as so many siphons to pump up the juice, which presents itself to those filaments; and these filaments are sufficient to afford all that the greatest part of trees want in the winter. But, being destined in the summer-time to more important offices, the tree spreads through the air those numberless fans, disposing them, in the most advantageous manner imaginable, to incumber each other as little as possible in pumping from the surrounding air all that they can absorb

forb from it, and to present, if I may so speak, this substance drawn from the common atmosphere to the direct rays of the sun, on purpose to receive the benefit which the influence of that great luminary can give it.

SECTION XIX.

On the power which vegetables have of absorbing different kinds of air.

IF a plant is shut up in a certain quantity of air, and all light hindered from falling upon it, it absorbs in general more air than it yields, and therefore the bulk of air
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is found less. The quantity of air thus absorbed by plants may vary from numberless circumstances, as well as from the particular nature of the plant. I have no time to search in my notes for all the particularities I have observed upon this subject. I can say in general from remembrance, that some water-plants were very willing to absorb a good deal of air, principally when they were placed with roots and all in the air; and that they readily absorbed air fouled by breathing.

One of those plants had also absorbed a great deal of dephlogisticated air, so that in one night it had absorbed half the quantity I had put with it, which amounted to 4 ounce measures, if I rightly remember.

This

This absorption also takes place in the day time; but as the plants at that time yield themselves a great quantity of air, the absorption is not so easily ascertained.

SECTION XX.

On the best manner of judging whether the plants are ready to yield their dephlogisticated air.

AS the light of the sun, and not the warmth, is the chief cause, if not the only one, which makes the plants yield their dephlogisticated air, it seems reasonable to think, that in a bright sun-shiny morning
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the plants will be earlier revived in their office than when the sun is hid by thick clouds. I found this difference to be very remarkable, so that in a dark cloudy morning I found the plants to begin their daily operation an hour or two later than in a clearer day. I even found that all the plants in the same garden did not awake, if I may so express myself, at the same time from their nocturnal stupor. Those plants, whose exposure was such as favoured the rays of the sun being cast early upon them, were revived earlier than those which were shaded by other plants, a wall, a house, &c. Nay, I even found that there was some difference in this respect between the leaves of the same tree; as I found those which were the first influenced by the
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the light of the sun, the first ready to operate ; when those of the opposite side of the tree, shaded by the first, were still in their state of stupor.

A ready way to know this time exactly is to put a leaf or two, from the plant you are to examine in this respect, in a glass full of fresh pump-water, and to observe, whether the bubbles appear upon them about as quick as they use to do in the full day time. If they do, you may be sure they are fit for the business.

But there is a readier way to know exactly this article of time, which I found by the water in the jar in which the green matter, discovered by Dr. Priestley, is already formed. The doctor found that this green matter yielded air bubbles

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only when placed in the sun ; which observation served me as a good index, whether those plants which have experienced nearly the same influence of the sun as the green matter, were fit to yield dephlogisticated air. The more brisk you see these bubbles rise, the quicker your plants will give theirs.

But this manner of judging can only be of service in the morning ; for in the middle of the day all leaves of plants, even those which were kept in a very dark place, revive so quickly, that they seem not to stand short in an observable way with those which were constantly in the open air.

SECTION XXI.

Conjectures why some waters, as distilled, boiled, and some other waters, do not promote, but impede the operation of the plants in yielding dephlogisticated air.

AS I think I have proved clearly enough that the dephlogisticated air yielded by plants is air elaborated by a kind of vital motion, carried on in the substance of the leaves, and kept up by the influence of the light of the sun, it seems that no more is required to collect this air than to prevent its diffusing itself through the common mass of the atmosphere. Water seems the most appropriated body for such an intention, for it is not hurtful to plants.

Many of them even thrive the best in it. The best quality required therefore in the water used for this purpose seems to be, to possess of itself air enough, so as not to imbibe it readily from the plants; and not so much as to be overcharged with it; for if the water is too much deprived of its own air, it must be more disposed to absorb it from bodies plunged into it. And if water should be so much impregnated with any air, this air would readily rush into the substance of the leaves, and spoil by its bulk, or by its particular nature, the elaboration of the dephlogisticated air; the more so, as water, when found saturated with air, is found to possess this air in the form of fixed air, which differs too much from the nature of dephlogisticated air,
or

or atmospheric air. Besides, water overcharged with air parts easily with it, which of consequence will of itself settle in the form of bubbles upon the leaves, and thus disturb their whole operation. We know that pump-water possesses of itself a great portion of air, which is generally thought to be for a part fixed air, to which it owes its agreeably pungent or brisk taste, which makes it palatable above all other waters. We know with more certainty, that boiled and distilled water are deprived of the greatest part of their air; and this is perhaps the reason, why they are not so palatable as common spring or pump-water. Therefore it seems to be not quite improbable, that water which has been boiled or distilled is very apt to absorb itself the air

which oozes out of the leaves, and that thus less air is gathered at the top of the bottle. This conjecture will perhaps find more ground from the following experiment. I placed some leaves of a vine in water, which I had, for this experiment, impregnated with fixed air: they were scarce under the surface of this water, but they were all covered with air bubbles; which seems to me to depend partly upon this water refusing to absorb any air issuing from the leaves, because it was already overcharged with air itself. It is true that any other body, plunged in water impregnated with fixed air, will also become covered with air bubbles; but these bubbles do not appear so soon, or increase so rapidly, as those of the living leaves. So that it seems, that the bubbles of
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the leaves increase faster because they are pushed out of the leaves by a vital motion in the leaf. It is also true, that leaves thus placed in water impregnated with fixed air, do not yield that fine dephlogistified air which they yield when placed in common pump-water; which may be owing perhaps to the great abundance of fixed air penetrating the leaves, by being absorbed, and oozing out as it were, in a kind of tumultuary way, together with the air already contained in the leaves. Thus the air issuing out of the leaves may not have undergone that degree of elaboration required to change it into dephlogistified air: for the least circumstance may disturb nature in this work; the shade of a building, or of another plant, may change this wonderful

operation, so as to produce quite the reverse, and to obtain a poisonous air instead of dephlogisticated air: for the evaporation of bad air in the dark depends on the vital motion within the plant, which, being not influenced by the light of the sun, produces a contrary effect. Thus a plant growing in an absolute darkness is without green colour, and fruit without the influence of the light has no flavour.

SECTION XXII.

Some remarks on the green matter which settles at the bottom and sides of the jars in which water is left standing.

THIS green matter, which seems to be of the vegetable kind, was first found by the Rev. Dr. Priestley to yield very pure dephlogisticated air: but it ceases at last to yield more air if the water of the jar is not renewed, which ought therefore to be done now and then.

It is wonderful that this matter seems to be never exhausted of yielding dephlogisticated air, though it has no free communication with the common atmosphere, from which

the most part of other plants seem to derive their stock of air. Does this vegetable matter imbibe the air from the water, and change it into dephlogisticated air? This does not seem to me probable, for I could not obtain from water, even by boiling, so much air as the water in which this substance was produced yielded by itself. I should rather incline to believe, that that wonderful power of nature, of changing one substance into another, and of promoting perpetually that transmutation of substances, which we may observe every where, is carried on in this green vegetable matter in a more ample and conspicuous way. The water itself, or some substance in the water, is, as I think, changed into this vegetation, and undergoes, by the influence of the sun shining

shining upon it, in this very substance or kind of plants, such a *metamorphosis* as to become what we call now dephlogisticated air. This real transmutation, though wonderful to the eye of a philosopher, yet is no more extraordinary than the change of grass and other vegetables into fat within the body of a graminivorous animal, and the production of oil from the watery juice of an olive tree. More examples are to be seen of such wonderful transmutations of sublunary beings in the article upon the mutability of air.

On purpose to obtain in a short time a great deal of dephlogisticated air from this green matter, I gathered a good deal of it from the sides of a stone trough placed near a spring upon the high road, and
always

always kept full of water for the horses. I put a good deal of this substance in a jar holding a gallon of pump-water, and inverted it in an earthen pan. In a week's time I found about $1\frac{1}{2}$ pint of very fine dephlogisticated air collected in the jar, which surpassed in purity the air obtained in another jar from the green matter generated by itself. See experiment 100.

SECTION XXIII.

In planting trees for rendering the air wholesomer, it seems not to be quite indifferent what kind of trees are made use of.

AFTER what is already said on the subject, there will be no doubt left, that vegetables have a remarkable share in cleansing and purifying our atmosphere. But as it seems to follow from my experiments, that some trees yield by the day a purer dephlogisticated air than others, and that some seem to be less disposed to infect common air by night, it can scarce be considered as a matter entirely indifferent what kind of trees ought to be planted,
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if the salubrity of the air was the chief object of such a plantation. I made some experiments for this purpose, of which a few are placed in the second part of this book. But I am far from thinking myself intitled to decide any thing upon this head; the more, because all trees co-operate to the same end, and because the œconomical advantage arising from the preference of one sort of tree above another may be thought to over-balance the small advantage to be derived from its superiority in rendering the air purer. I must content myself with the discovery of the fact, and leave the rest to others, who, by farther and more decisive experiments, may have a better right to decide something upon this head than I can as yet pretend to.

SECTION XXIV.

The largest and the more perfect leaves yield more and purer dephlogisticated air, than those which are not yet full grown.

NOTHING seems to me a more convincing proof that the elaboration of dephlogisticated air is an effect of a kind of vital motion in the texture of the leaves, than that young leaves, not yet grown to their natural size, yield their air-bubbles slower and less in bulk, and that the air yielded by full-grown leaves surpasses in purity that which is obtained from leaves not yet come to perfection.

It is an amusing sight to observe in a jar full of pump-water the extremity of a branch of a vine, which contains leaves of different ages, from the maturest to those which only begin to unfold themselves. The air-bubbles make first their appearance upon the old leaves, then upon those that follow, and last of all on the new-born ones. The same proportion takes place also in the size of the bubbles; the largest or oldest leaves having always the largest bubbles, and therefore yielding far the greatest quantity of dephlogisticated air.

As it seems to be almost a constant rule, that the leaves which yield the greatest quantity of air, yield also the purest; the same rule also takes place in the old and new leaves. The young leaves
seem

seem not to have their organization completed for the office to which they are destined, and therefore they are not yet able to elaborate so much nor so good dephlogisticated air as the old ones. The experiment 122 and 123 seem to be decisive in this respect.

SECTION XXV.

Though the diminution of the bulk of nitrous air is believed to be an unquestionable test of the goodness of any air, yet, it must be allowed, that in some kinds of airs this test may fail.

AFTER having tried a great variety of airs myself, and after
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having seen many more tried by Abbé Fontana, I no longer made the least doubt, but the discovery of Dr. Priestley in judging of the exact degree of goodness of any air was without any exception. But, as I was resolved to abstain as much as possible from all analogical conclusions, without they were supported by direct experiments, I tried every air I could find, not only by the nitrous test, but also by the flame of a candle, without, however, harbouring any mistrust in the already adopted manner of examining the degree of goodness of them.

I had already been convinced, that inflammable air was made explosive in a few hours when exposed in the sun with any plant, though I sometimes found it, by the nitrous test, so much corrected as to approach

proach near to the goodness of common air. This gave me some suspicion, that this inflammable air might be susceptible of a still more remarkable correction or purification, at least in appearance, without losing its explosive quality.

On purpose to discover the whole, I left some inflammable air upon *persicaria*, and some upon walnut leaves, during forty-eight hours, keeping the jars continually in the open air.

I tried first the air of the jar in which the walnut leaves were, in the manner familiar to Dr. Priestley and in that of A. Fontana ; and repeated each trial twice with the same result. I found the air by both these methods to exhibit all the appearance of air, superior in quality to common air ; as may be seen in

experiments 110, 111. 113, 114, and 115; and yet I found this very air to explode with such a loud report, even in a cylindrical jar, that my servant, who kept the glass in his hands, thought it was absolutely broken. This event gave me no small concern for a method of trying the goodness of airs, which I had already considered as infallible. However, I had still some hopes left that I had committed some blunder in this experiment; and very luckily I had still at hand the jar which contained the *persicaria urens* with the inflammable air; but I was sorry to find that my suspicion was but too well grounded; for this air gave, by two different trials, the following result: one measure of it with one of nitrous air occupied 0.95; and with two measures

measures of nitrous air 1.92; by A. Fontana's method it gave 1.00, 1.96, 2.95; and thus it did appear by these trials to surpass far in goodness the common air; and yet it exploded at the flame of a candle with an uncommon loud report. See experiments 110 and 111.

There remained still one experiment to be tried with this air, *viz.* to put a living animal in it. I was sorry to have spent the most part of this air, so as not to have enough of it left for this trial. However, I was resolved to push the experiment farther, and to let the inflammable air stand a longer while upon the plants, before it was to be employed for the different trials, and principally before an animal should be put in it. Some entire plants of *persicaria urens* were put

in a gallon jar full of water, and as much strong inflammable air was let up as to fill above one third of the jar. I left it in the garden during six days, when I found, to my surprize, that it was very far from being corrected; for one measure of it, with one of nitrous air, occupied 1.80; it gave the following result by Abbé Fontana's method, 2.58, 3.58: a chicken, near three weeks old, died in it in the space of one minute.

This result, so different from the former, greatly puzzled me, and restored my hope that the nitrous test was without exception, and that I must have committed some error in the former experiment.

I was, however, far from giving up entirely my suspicion of the failure of the nitrous test. I re-

solved to repeat the experiment again, with all possible attention; I had still half a pint left of the inflammable air, which had been during six days upon the *persicaria urens* without being much mended. See exp. 112. I put a fresh plant of mustard in a jar filled with water, and let up this inflammable air in the jar, so that the plant was in contact with the air. I placed it in the garden on a Saturday at twelve o'clock. I tried this air the next day between one and two in the afternoon, and found it by the nitrous test so much mended, that it appeared better than common air, and yet it exploded with a loud report by the approach of a candle. I replaced the jar again in the garden, and put the same air again to the nitrous test on the Monday following, when it appeared to

be far superior to the atmospheric air, for one measure of it, with one of nitrous air, occupied 0.96; and yet it exploded as strongly as before. I replaced it again in the garden during four hours more, when it appeared to be still farther improved by the nitrous test, without losing, however, in the least, its explosive nature. See experiment 115.

I had also on the same Saturday put some plants of *persicaria urens* with their roots in a jar full of water, and let up two pints of strong inflammable air. I found this air on Sunday, after the jar had been 24 hours in the garden, so much corrected, that it approached to the goodness of common air by the nitrous test, though it exploded with a loud report. I replaced the jar again in the garden, and again examined

amined the air on Monday between one and two o'clock, when it appeared, by the nitrous test, about as good as common air; and yet it had not lost its explosive quality. After this, I replaced the jar as before, in the garden, and put the same air again to the test between four and five in the afternoon of the same day, when it appeared to be better than common air, without having lost its explosive force. There remained now nothing more to be done, than to try the effect of this air upon a living animal. I placed a lively chicken, three weeks old, in a jar filled with this air: it grew sick directly, and was in six minutes near dying, when I took it out quite motionless. It remained in the open air during several minutes in a dying condition, after which it gradually recovered.

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I was now thoroughly convinced, that the nitrous test failed entirely in shewing the degree of salubrity of this air ; for it appeared by this method to be nearly dephlogisticated air, and yet it was still a true poisonous air*.

I was indeed very sorry to find this failure in a method so well adapted for the exploration of atmospheric air. But I am very far from thinking that this exception diminishes in any way the real value of the important discovery, that *Nitrous air diminishes respirable air in the proportion to its salubrity*. For this test holds good in atmospheric air, which is the chief object of our experiments.

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* Does this air owe its explosive nature to the dephlogisticated air oozing out of the plant? But this very air becomes also explosive, though it stands with a
plant

SECTION XXVI.

Air is one of the most changeable substances in nature, and appearing under very different forms and qualities from a variety of causes.

THE air of our atmosphere is seldom during a whole day of the same quality. Its degree of wholesomeness is perhaps not less subject to variations than its weight and its degree of heat and cold. The barometer indicates the first, and the thermometer the other. But those two instruments seem to have no relation to the more or less purity of the atmosphere, or the more or less fitness of the air for the use of respiration.

plant during the night, when the plants yield but a very small quantity of bad air. So that it rather appears to be changed by the plant in a kind of simple explosive air, or a true *fulminating air*, the only yet discovered, as far as I know.

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The invention of an *Eudiometer*, or of an instrument or contrivance, by which the degree of purity of the common air, or its fitness for respiration, or rather its wholesomeness, can be investigated just as well as its weight, and its degree of heat and cold, is perhaps one of the most extraordinary inventions which ever was made.

We owe this important discovery to the Rev. Dr. Priestley. He found that nitrous air has the singular property of diminishing, or of being diminished by, common air in proportion to its goodness; or that the bulk of the two airs joined together contracts itself in a so much the narrower space, as the common air is better, purer, or more fit for respiration. It will soon appear to what a considerable degree of accuracy

racy the Abbé Fontana has brought this truly great discovery.

We have now in our hands the means of judging, not only of the degree of goodness of the common air upon the spot, but we may with as much ease also judge of the quality of the air of any country, by sending the air of it in close bottles. But as the air upon the same spot undergoes itself continual changes, we can but very seldom expect an accurate agreement of two experiments, unless made at the same time, or unless a quantity of the same air be shut up in a bottle sufficient for different experiments.

Until accurate instruments fit for such purposes are generally known, and employed with all the attention required, we shall not be able to judge of that degree of goodness
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which the air possesses for the most part of the year in a country, and thus to determine the advantages which would arrive to our constitution, in spending our lives in one country rather than in another, on purpose to preserve a good state of health, to cure particular diseases which require a pure air, or to protract our existence in this world in particular bodily dispositions. We must as yet content ourselves with the amusement of the experiment.

The continual changes which I observed in the atmosphere daily, by trying its constitution, convinced me of the too precipitate judgment of some philosophers, who, though furnished with but indifferent instruments, have begun already to assert the degree of goodness of certain places, by one or two observations

vations made in the time they passed through such a place. But I must leave the discussion of this matter to my respectable friend Abbé Fontana, who, in my opinion, has cast a great light upon this important subject; and intends soon to publish his observations on this head. I will add only some further reflections upon the changeableness of air, its Proteus-like and metaphorical nature.

Since the experiments of the Rev. Dr. *Hales*, we know that air enters the composition of bodies, and even serves as a kind of cement for the stronger cohesion of the constituent particles of a solid body. By this it seems that air may become itself a solid body, as it constitutes such a considerable part of some particular bodies, such as are, for instance,

instance, vegetables, calcarious stone, nitre, &c. That a fluid body may become a solid, is nothing extraordinary ; we see that water becomes as solid as a stone, and remains so, in a place sufficiently cold. There are perhaps in the world no substances which are by their nature *fluid* : for all substances yet found may be, by different operations, principally by a sufficient degree of heat, rendered fluid ; and all fluids may be changed into solid bodies by applying to them a sufficient degree of cold. Mercury itself was rendered as malleable as any other metal, by Professor *Brown* at St. Petersburg, by a very great degree of cold.

Since that kind of air is known, which goes now under the name of fixed air, and which *Van Helmont*,

mont, called *Gas Sylvestre*, it has been imagined by many, that different vegetables contain almost nothing but fixed air, because they saw that as soon as they began to ferment they emitted really fixed air. But if from this we conclude that this very same vegetable contained this fixed air, as such, concealed in its substance, and existing there, as it were in a concentrated or compressed state, almost as common air is in a condensing engine before the fermentation began, we may possibly make an erroneous conclusion; for it may be that this vegetable did not contain more fixed air as such than inflammable air; but that a part of the substance of the vegetable has undergone such a change by the action of the fermentation as to become what is now called

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fixed air, but what it was very far from being before the fermentation. That this may be the case I was induced to suspect by the following experiments : I squeezed the air out of different vegetables, keeping them under water, such as malva, potatoe-plant, hyoscyamus, apples, &c. I expected to find the most part of this air fixed air : but I was much disappointed ; for this air was not diminishable by shaking it in water. By examining it in another way, I found that the flame of a wax-taper would grow dim in it, and that it was only somewhat inferior in quality to common air ; for one measure of this air drawn from an apple, with one of nitrous air, occupied 1.24 ; and that expressed from the leaves of hyoscyamus occupied 1.25. The air expressed from malva and
potatoe-

potatoe-plants appeared to be somewhat better. This air is undoubtedly the very air of the vegetable unaltered. I placed all those vegetables separately near the fire in water, and by examining the air disengaged from them I found it to be of a much worse quality than that which I obtained by squeezing; and by trying the air extracted from them by actual ebullition, I found it to be poisonous, and to extinguish flame. The air from an apple obtained by boiling was so bad, that one measure of it with one of nitrous air occupied 1.71. Now these very plants, placed in the same water in the sun-shine, yield very fine dephlogisticated air, and by fermentation they yield fixed air. Is it therefore not probable, that the very air contained in the plant in

its natural state was really an air approaching in quality to common air; and that the heat of the ebullition had changed this very air into phlogisticated air, in the same way as the act of fermentation changes it into fixed air, the light of the sun into fine dephlogisticated air, the digestion in the stomach and the intestines of an animal (a great deal of the air contained in the intestines, and all that from which we ease ourselves by the rectum, is pure inflammable air) and actual fire applied to it into inflammable air, and the obscurity of the night into another kind of truly poisonous air? Could it be said with any degree of probability, that one and the same vegetable contains these six kinds of air, so different in their nature, and even contrary to one another?

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Is it not more reasonable to say that vegetables contain an air, or by whatever name you will please to call it, which by undergoing different operations changes into different sorts of air?

Whoever therefore says, that such or such substance contains such or such air, because he extracts such air from it by the action of fire, by fermentation, or by any other means, may speak erroneously.

Nitrous acid, or spirit of nitre, yields nothing but nitrous air when it is poured upon mercury, copper, iron, &c.; but, when it is mixed with iron filings in a very diluted state, it gives, by the assistance of a moderate degree of heat, a mixture of different airs, partly fixed, partly common air, and partly phlogisticated air, (which experiment I saw

at Abbé Fontana's). When this very acid is joined to some earthy substance, or to a vegetable alkaline salt (with which it constitutes nitre), it yields by the action of the fire nothing but pure dephlogisticated air, in such abundance, that the quantity of it is equal to about eight hundred times the bulk of the nitre, as Abbé Fontana found.

Such-like transmutations which air seems to undergo are every where obvious in nature. All bodies upon our earth, or almost all, undergo continually some alterations, and at last deviate entirely from what they were before. The plant which affords us the most wholesome food is perhaps the next to another which draws out of the same spot of ground a poisonous juice. The food by which a viper
lives

lives changes within his body into a substance which has nothing deleterious in itself, but in one place of its body a most virulent poison is elaborated from it. The same juice which the root of a tree pumps from the earth is changed into various fruits, very different in taste and qualities, if different sort of fruits are grafted upon it. An animal body becomes a manure for plants by corruption ; it changes thus in the substance of a vegetable ; this, being burnt, changes into ashes ; which, by the action of the same fire, and by the addition of some sand and some calx of lead, changes into fine transparent glass. Thus what is now a part of our body may become in a short time a part of a pot or bottle.

The three mineral acids themselves may possibly be but one, and the same acid modified by some particular addition, which time may discover, to separate and thus to change marine acid into nitrous acid, &c. Some eminent chymists have already asserted this as their opinion. More or less phlogiston in one acid than in another may make the one quite different in nature from the other. Common air impregnated with phlogiston makes a poisonous air; and common air, deprived of it, makes dephlogistified air; in the one an animal dies in a little time; in the other it lives four or five times longer than in common air. Vitriolic acid extracts from iron its phlogiston, and allows it to impregnate the air disengaged in the act of solution.

Nitrous

Nitrous acid disengages also the phlogiston from the iron, but does not allow it to pass in the air disengaged from it, so as to make it inflammable. It seems to keep this phlogiston to itself; for it is, after the solution, no more to be found in the dissolved iron, when precipitated in the form of ochre; but the same spirit of nitre, when dissolving iron in a very diluted state, leaves the most part of the phlogiston with the metal, and rises in the form of partly fixed air, common air, and what is called phlogisticated air, as was said above; and by this method iron may be reduced to the most impalpable powder, all obedient to the magnet, which is a method of making *Æthiops Martialis* of great importance for medical uses, and was discovered by an apothecary of Paris.

Paris. Vitriolic acid extracts from calcareous earth, fixed air, and from some kinds of sparrs an air of a wonderful quality, corroding glass itself, which seems to be almost an incorruptible substance, and reducing it into dust by its contact only; and this air, so active upon glass, is by the first approach of water immediately reduced again into the form of the stone out of which it was extracted.

Considering all what is said before, I incline much to the opinion, that the various kinds of air extracted from the different bodies owe, for a great part, their specific nature to the transmutation which they undergo in the operation by which they are obtained; and that they cannot, at least not all, be said to exist in the body in a contracted state.

state with more propriety than that glass exists actually in our body, because, by the action of fire, our body may be changed in a constituent part of that substance; and that fat exists in grass and other vegetables, because in the organs of an animal feeding upon these herbs they are partly changed into fat. Thus, when we feed upon vegetables, we do not in reality take in fixed air, existing as such in the substance of that food, and only let loose or extricated in our bowels; but it is more probable, that such food, undergoing in our stomach and intestines a kind of fermentation, yields really fixed air, not extricated, but generated by the act of fermentation.

As we have seen now, that common air is far from being an unalterable fluid, only to be changed
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by the addition of something, or by becoming deprived of something extraneous to its own original simple nature; we can no more be surprized to find, that the constitution of the atmosphere remains seldom a whole day the same, and that the degree of salubrity is continually changing. Indeed, in the course of three months, which I spent in my solitary retirement, I scarce found the degree of salubrity of the common air just the same during two days.

Those who are not yet acquainted with the accurateness of Abbé Fontana's new *Eudiometer*, will be much inclined to believe, that the appearance of such continual variations is more owing to the imperfection of the method of exploring the air, than to the real changes happening in

in our element: and, indeed, I was much of that opinion, till Abbé Fontana convinced me of my error; for, by keeping a bottle full of air taken from the atmosphere at the same time, the constitution of it is explored and accurately registered; and examining some time afterwards this very air, closely shut up in a bottle, you find the result of the trial to correspond exactly with the result of that which was made at the time when the air was taken from the atmosphere, and by no means conform with the result of the trial instituted with the common air of the day, unless it should happen that the constitution of the atmosphere was just the same at both times. I take this to be a demonstrative proof of the excellence of this method, as well as of the

the erroneous judgement which any body might form of the accurate degree of goodness of the air of any given place, by examining it once or twice with nitrous air, principally if the observer is not in possession of an accurate instrument for making such an observation, or if he has not observed to the greatest nicety all the manoeuvres in the time of making the experiment.

It would be a difficult task to discover as yet the true causes of that continual fluctuation in the degree of salubrity of the air in the same place. But it seems to me not improbable, that this inconsistency is to be attributed in general to the natural changeableness of the air itself, by which it undergoes continual alterations from a variety of causes, of which a great number

number are perhaps not to be traced by human sagacity; and, indeed, if the air of a vegetable is from the nature of common air, or air approaching it, changed into true poisonous air, by applying only heat to it, as I have said already, and that some more or less light to which a plant is exposed changes its natural air into the most salubrious or the most poisonous air, may it not be suspected, with some degree of reason, that a great variety of causes, which have been till now overlooked, and which vary themselves continually, may bring on a very material alteration in our atmosphere, such as, for instance, heat and cold, dryness and moisture, light and obscurity, which I have already demonstrated to affect the operation of vegetables upon the

6 air,

air, winds blowing from different quarters, and conveying airs of different qualities, from distant countries, and many other operations of nature, unnoticed as yet?

Water itself, one of the simplest and the most unalterable substances known, seems to be changeable into dephlogisticated air, or at least to contain some things which may be transformed into this air by the influence of the day-light; for the green vegetable substance, which serves as a kind of laboratory, in which this salubrious air is produced, is formed from the water itself. Abbé Fontana made a great many experiments tending to examine the air extracted from different waters by heat. I was present at these experiments in the summer of 1777, being then at Paris. He extracted
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from water of the Seine, and of the aqueduct of Arcueil, an air better than common air, which was a step towards the discovery of still better air from simple water, by some other way not yet hit upon. These interesting experiments are printed in the *Journal de Physique de l'Abbé Rosier*, May 1779.

SECTION XXVII.

On the nature of the air oozing out of our skin.

AS our bodies perspire continually a watery liquid, either in an invisible way by what is called insensible perspiration, or by way of
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sweat,

sweat, so a quantity of air seems to issue continually from the pores of our skin. This is easily to be observed in a cold or warm bath, in which we may clearly see whole bubbles of this air rising upon the skin, and at last rise to the top of the water. By plunging the hand and arm even in cold water, we may immediately observe a large number of those bubbles every where: and they are the more apparent when the skin is thoroughly dry before the part is plunged into the water; and much more so when it is plunged precipitately into it.

It is however to be observed, that all the air contained in those bubbles, which appear upon the skin, when a part of our body thoroughly dry is on a sudden plunged under
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the surface of the water, is not such as really issues out of the pores ; for, as our skin is always covered with some unctuous matter which seems to repel water, the sudden immersion does not allow the water to chase before it all the air sticking, as it were, to the skin, but a good deal of it is left upon it, and forms partly these large bubbles. This seems to be the more probable, as particularly those places to which these bubbles adhere are found quite dry, if observed attentively, when the part is withdrawn out of the water. But these very bubbles are in all probability also partly owing to air oozing out of the skin ; for, if they were nothing but atmospheric air, they would not increase in size in cold water, but become smaller by con-

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denfation : now they increafe even to a very large fize in the coldeft water, and at laft detach themfelves from the fkin. A warm bath is not very proper to obtain the air oozing out of our fkin. Water having been warmed has loft a good deal of the air naturally contained in it, and thus is very apt to abforb the air oozing out of the fkin. The beft water for this purpofe is pump-water frefh drawn.

If we keep our arm, or any other part of the body, under water, and rub off all the air bubbles fticking to the fkin, we fhall fee in a little while a great many fmall ones fucceed. But the eafieft way to convince one's felf of the continual oozing out of air from our fkin, is to rub the fkin with the edge of an inverted glafs full of water, and
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long enough to keep a good part of it above the surface of the water in the time the brim of it is sliding under water over the skin. In this way one may see an immense number of very minute bubbles rise continually to the top of the water in the inverted glass, and gather in larger bubbles at the top. By this method I collected, in a little time, from my arms, a measure of this air, which seemed to be partly fixed air, as it was somewhat absorbed by the water; at least, I thought to find the mass of it always less than it was before. This air put to the nitrous test was found far from being good respirable air; for one measure of it, with one of nitrous air occupied, 1.46.

I took a quantity of air in like manner from the arms of a

healthy person, 19 years old, and found one measure of it and one of nitrous air to occupy 1.84; which convinced me that the air evaporating from the skin of young people is not purer than that emitted from the skin of people more advanced in years; and that if there should be any advantage for old people to sleep in the same bed with young ones, as some imagine, it cannot likely be owing to their perspiring a better and wholesomer air from their skin. It is a very erroneous opinion, and even tending to do material mischief, that the air of a room, in which a great number of young people have been shut up, as in schools, is become very wholesome for old people to breathe in. I have seen school-masters so strongly prejudiced with this notion, that

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they

they even would not allow the windows of the school to be opened, for fear that the young air, as they called it, of the school-boys should escape; thinking that breathing this infectious and truly noxious evaporation would prolong their own life.

As I found that the bubbles appearing upon the skin, when a part of our body is plunged under water, are so much the larger as the part is put the more precipitately in the fluid, I could scarce doubt but the air gathered from these large bubbles must be for a great part atmospheric air, which could not so quickly detach itself from the skin by the suddenness of the immersion; and I expected, therefore, that this air would give by the nitrous test a better appearance than that

gathered from the small bubbles scraped from the skin by the edge of a glass. I gathered from another young and healthy person the air of the large bubbles found upon the skin, by plunging the arm suddenly under the water. And I found it approaching more to the nature of common air, though a candle could not have burned in it, nor an animal breathe in it without anxiety; for one measure of it with one of nitrous air occupied 1.49.

CONCLUSION.

I HOPE the indulgent reader will excuse in me a small degree of vanity, in flattering myself with having discovered a law of nature hitherto entirely unknown, and hid till now behind the screen of that awful darkness which overcasts our earth during the time it withdraws its surface from the direct influence of that all-reviving luminary, the sun.

I flatter myself also to have put beyond all doubt, that the vegetables have a remarkable share in keeping up the salubrity of our atmosphere, by imbibing those septic, noxious, and phlogistic particles, which were communicated to it by
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the breathing of so many animals which inhabit the surface of the earth, and by many other causes ; as well as by pouring down a most beneficial shower of purified or dephlogisticated air, which, diffusing itself through the mass of common air, counteracts the general causes, tending to contaminate our atmosphere, and to render it unfit for the use of respiration. I was lucky enough to discover that the *vegetation* itself of the plants has nothing to do with the cleansing our atmosphere ; but that this great work is operated by the influence of the sun's light, exciting and keeping up the vital and intestine motion of these numberless fans, which the most part of plants display at once, just at the time when the general tendency

tendency to corruption is increased by the increase of heat.

Though we are too much accustomed to look upon the most obvious operations of nature with a kind of unconcern and indifference, such as, for instance, the vegetation of plants ; yet we cannot look with so much indifference upon the final causes of those every where obvious scenes when we discover them ; for they do not so much affect the organs of our sight and other external senses, as they do our understanding, our reason, our judgment ; by which only we are superior to all other living animals. The consideration of final causes gives us to understand that this great *universe* is not the offspring of chance, not coëval with the beginning of time, or of an eternal origin ; but that it
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has been made by an Omnipotent Being, who, by giving it existence, has, at the same time, endowed it with most wonderful qualities and powers, continually in action, and tending with an astonishing harmony to one general end, the preservation of the whole.

An upright mind, averse to that manner of living which induces many to wish, rather than really to believe, that this world is not superintended by an intelligent Being, takes delight in finding out those deep designs, which, by their obvious tendency to promote the preservation of the whole, inspire him with that awful reverence we owe to the Supreme Cause of every thing, and fill him with that consoling expectation, that the only being upon earth capable of true reason, and

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of tracing the existence of a God in his wonderful works, and of contemplating him in adoration, may expect not to be entirely annihilated after his body is returned into dust, out of which it took its origin.

But to come back from this digression to the purpose, let us consider how much the real facts drawn from nature itself are concordant with the theory deduced from my experiments. If the leaves of vegetables have really a considerable share in cleansing the atmosphere, it must happen, that the time, when our common air is the purest, is the summer and the winter; for in the summer the plants are in their greatest vigour; and in the middle of the winter the causes of general corruption are the most

most checked by the cold. Now this is just what happens. As soon as in the advanced autumn the leaves begin to wither and to fall, and to contribute even somewhat themselves to contaminate the air by their corruption, the degree of purity of the atmosphere is really less than it was during the time of the summer; and this atmosphere does not return to its former good quality till the winter is set in, and till the remaining tendency to corruption is checked by the increase of cold. In the spring, when the sun begins to promote somewhat the general tendency to corruption, without having yet influence enough upon the vegetables to make them display their leaves, the common air begins to be less fit for respiration, till it returns again to its former

mer purity as soon as the leaves are produced. And this is what Abbé Fontana found to be a constant fact.

If I had more leisure, I should be inclined to expatiate in a wide and open field of reflections, which present themselves to my mind, and to draw all the consequences which seem to flow, as from a fountain-head, from the already mentioned observations.

Is it not probable, that those who labour under consumptive and asthmatic complaints, and who find the greatest relief, and sometimes a perfect cure, by retiring to mild climates, where vegetation is lively, and begins sooner in the spring, should go to such places where the constitution of the air is found by experience to be during the whole
year

year the best? But these places will not be known till some accurate method of examining the goodness of common air be in general use.

Is it not somewhat probable, that it is unsafe for the health of people to sleep in rooms having windows towards a small open place crouded with the branches of a large tree, so hidden from the influence of the sun as to receive but seldom its rays? I remember to have heard people say, that it was unwholesome to sit under a wallnut-tree, and that they found themselves affected by its shade. But I looked upon such an apprehension as one of those popular or vulgar errors which are propagated from father to son. I should now be inclined to think, that an apprehension of some mischief might not be entirely ill-grounded,

grounded, when such a tree stands, as is often the case in a narrow yard confined by the surrounding buildings.

It is a general belief in the West Indies, founded upon constant experience, that the mangeneel-tree *Hippomane Mancinella* (Linn. Spec. Plant. 1431) throws out very hurtful exhalations, so as to endanger those people, who, ignorant of the nature of this tree, venture to lay down under it.

The plant called *Lobelia Longiflora*, growing also in the West Indies, spreads such deleterious exhalations from it, that a considerable oppression is felt upon the breast in approaching, at several feet distance, this plant, placed in the corner of a hot-house or room. (See the description

scription of this plant in *Jacquini Hortus Botanicus Vindobonensis*).

The plant called *Dictamnus Albus*, or *Fraxinella*, which is by no means rare throughout almost all Europe, when in flower spreads round about inflammable air, which, by the approach of a candle by night, flashes as other inflammable air does. We know that an animal breathing in this kind of air loses its life: so that if a man was to sleep with his head in the middle of the branches of this plant, he might run a risk of being killed by it.

May we not ascribe the unwholesomeness of the air of that immense plain in which *Rome* stands to the want of trees and other vegetables? That very plain was, in ancient times, reputed to be a very wholesome country, when it was well cultivated
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and inhabited. And in our days, being not far from a real desert, it is so notorious for being unwholesome, that the people of the country think it highly dangerous to pass a single night in it, even in the middle of the summer; whereas in Tuscany, which is peopled and cultivated to the utmost, one may sleep the whole summer in the open air without fearing more injury from it than from the air within the house. The Pontine Lake, *Lacus Pontinus*, in the dominions of the pope, in which formerly, when cultivated, were numbers of inhabitants, supplying Rome with the best productions of the earth, is at present a most dismal desert, spreading round about it unwholesome and deadly exhalations, so that scarce any living animal can breathe this air with-

out soon losing its health, and finding its destruction.

The want of proper cultivation contributes, perhaps, not a little towards rendering the immense plains of Hungary less wholesome than they would otherwise be. The country round about Vienna is perhaps likewise in want of a sufficient number of trees.

PART THE SECOND.

Containing a series of experiments made with leaves, flowers, fruits, stalks, and roots of different plants, on purpose to examine the nature of the air they yield of themselves, and to trace their effects upon common air in different circumstances.

SECTION I.

Introduction.

BEFORE I proceed to give an account of the various experiments I made during the course of

this summer, 1779, I must first acquaint the reader, that the method which I generally pursued, in putting the different species of air to the nitrous test, was the same which the celebrated Abbé Fontana makes use of now, and of which he himself has not yet given an account to the publick. As I had no right either to claim the invention of his method, or to anticipate the publication of it without his leave, I have asked his consent on this head. He agreed to my request very readily, gave me his notes to consult, and even permitted me to get his instruments engraved; for which purpose he allowed me to make use of his own drawings.

As he had already shewn me his method of examining the different kinds of air in regard to their degree
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of salubrity, or fitness for respiration, when I was with him at Paris in the beginning of the summer of 1777, and as I have, since he rejoined me in London, 1778, seen a very great number of the like experiments, I provided myself with the same instruments, on purpose to imitate his method of examining air, which I found so accurate, that, in ten experiments made one after the other with the same kind of air, the result differed seldom above $\frac{1}{500}$; that is to say, that the remaining bulk of the three measures of nitrous air, which he joins one after another to the two measures of atmospheric air, is so alike in the various experiments made with the same common air, that the difference will seldom amount to more than $\frac{1}{500}$ of the whole; which accuracy in ex-

ploring the degree of goodness of respirable air surpasses the exactness of judging of the degree of heat and cold by the thermometer of Reaumur.

The Abbé has, since I saw him at Paris, changed somewhat his instruments and method of using them, or rather corrected them a little; but they remain still materially the same as they were before.

I must beg the reader to stop here, and to cast his eyes upon the copper plate and the explication of the figures before he proceeds farther.

The new *Eudiometer*, or instrument for finding the accurate degree of salubrity, or fitness for respiration of a given air, consists of different pieces; two of which are the

the principal and absolutely necessary. One is a glass cylindrical tube, or the *great measure*, 18—20 Paris inches long, of an equal bore throughout its whole extent, whose diameter must be of about $\frac{1}{2}$ inch, or not much less, though it may be larger. This glass tube has divisions marked upon it, each of exactly three Paris inches. The inside of this tube ought to be rubbed with fine emery, to take off the smooth surface of the glass; for, if the surface of the glass be not a little rough, the water will remain here and there in the form of drops adhering to the inside of the tube, when air is let up into it; and thus so much of the space destined for the air is occupied by these drops, which renders the column of air longer than it would have been if
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the water had run down equally along the side of the tube; or at least it renders the column of air uncertain in length: each division of this glass tube is sub-divided into 100 equal parts, which are not expressed upon the glass tube itself, but engraved for convenience upon a brass slider or cylinder moving along the glass tube. This slider must be open on both sides, to shew the inside of the glass tube, that the height of the column of water in the tube may be seen.

The second necessary instrument is the *little measure*, consisting of a glass tube of a similar diameter with the great tube, and only three inches long. This small tube must also be made rough on the inside with fine emery. This little measure is fixed in a brass socket, hav-

ing a flat slider at the orifice of the tube, which, being pushed in when the tube and the socket are full of air, cuts off exactly the column of air within the tube, and at once shuts out that quantity of air which is more than the three inches wanted. All that part of the column of air which is thus cut off by the flat slider, is let out by turning or inverting the whole measure under the surface of the water. Thus the quantity of air shut up in this little measure will be constantly the same, whatever change may afterwards happen to the expansive force, or to the elasticity of the air within this measure.

Abbé Fontana uses this *Eudiometer* in the following manner: he first introduces two measures of the
 air

air to be examined, then one measure of nitrous air : at the moment the two airs come into contact with one another, he shakes the great tube in the water till both airs are thoroughly mixed together. This being done, the tube is put in the water-trough, in a position nearly vertical, to allow time for the water to run down along the inside of the tube, and to leave the column of air free. He then slides the brass scale upon the glass tube, till the 0, or the mark where the subdivisions begin, correspond with that spot where the two columns of water and air meet. He observes with what sub-division of the scale the next mark upon the glass tube above the column of water coincides ; which number he writes down. He then lets up another
measure

measure of nitrous air, shakes the tube in the moment the two airs come into contact, and, after some repose, he moves again the o of the brass scale to the place where the columns of air and water meet, and writes down the degree of the scale which corresponds with the next mark of the glass tube above the water. After this he lets up a third measure of nitrous air, and, after shaking and reposing as before, he marks also the degree of the scale corresponding with the next mark of the glass tube above the water, and thus finishes the whole operation, if the air examined is common air: for no more diminution of this air would happen if more nitrous air was added, as three measures of nitrous air are sufficient to saturate

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fully

fully two measures of any atmospheric air; he takes particular care to perform every experiment in the same manner, as well in the handling of the instruments, as in the exact time, even to a moment, of shaking the two airs together, of letting the tube stand by, before he examines the number upon the brass scale, &c.

After the whole operation is finished, he deducts the number of the sub-divisions of the whole column of air remaining in the tube from the number of all the sub-divisions or parts of both airs which were let up; and the result gives exactly the number of parts or sub-divisions which were destroyed: for instance, if, after the third measure of nitrous air being let up, the next mark of the tube corresponds with

with the number 8 upon the scale, and if above this mark are remaining three entire divisions of 100 partitions each of the column of air, the quantity of air existing in the tube amounts to 308 sub-divisions, which being subtracted from the 5 measures of both airs employed, or from the 500 sub-divisions of both airs, the remainder will be 192, which is the exact number of the parts or sub-divisions of the two airs destroyed.

If the air to be examined is dephlogisticated air, he continues letting up one measure of nitrous air after another in the manner mentioned, till no more diminution takes place. Six, seven, and sometimes eight measures of nitrous air are required to saturate two measures of dephlo-

dephlogisticated air if it be very pure.

What has been already said of this method of putting different airs to the nitrous test will be sufficient, I hope, to guide the reader in imitating it. But he will find in the result of every trial, made with the same species of air, so much difference, that he would mistrust the whole method if he did not observe every minute circumstance in the course of the whole experiment. It has cost the Abbé some years assiduous labour before he reduced this method to that degree of accuracy which it has now acquired in his hands.

Those who wish to perform this amusing experiment themselves will think it worth their while to look over the following necessary cautions

cautions to be observed, which I extracted from the manuscript of the author.

He reduces the various sources from which errors may arise in this manner of exploring air to twenty; which are, however, not all of equal importance, and may counterbalance in some measure one another, so that one error may correct another. But some of these are of such importance, that by overlooking them it may happen that the best atmospheric air shall appear to be a true poisonous one.

Those errors may originate principally either from the great tube or measure, or from the little measure.

The errors which may originate from the small measure are seven :

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I. The

I. The first error may be committed by the hand, which, by touching this tube in the time of filling it with air, may expand this air by communicating its heat to it. The result of this error may amount to two sub-divisions.

II. The second error may be committed also by the warmth of the hand in which this measure is kept, when it is raised till the flat slider is on a level with the surface of the water, in the moment it is pushed in to cut the column of air within the measure from the air to be shut out. This error may also amount to two sub-divisions.

III. The third error may be committed by not keeping the measure, in the moment of shutting the slider, exactly at the height required ; for, if the water within and without
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the measure be not on a level, the column of air within the measure may be more or less compressed. This error may amount to four subdivisions.

IV. The fourth error may depend upon the inside of this tube not being made rough by emery ; for water settles in the form of drops on the smooth surface of glass. These drops adhering here and there to the inside of this tube render its capacity greater or smaller. This error may amount to at least three subdivisions.

V. The fifth error depending on this measure may be owing to the difference of time between the filling this tube with air and the shutting the flat slider : for after the air is let up into this tube the water runs down its side for some

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time ;

time; so that the longer the interval between filling it with air and pushing in the slider is, the more the inside of this tube is cleared from water, and thus the more air it will contain. This error may also amount to three sub-divisions.

VI. The sixth error, which may be committed by the small tube or measure, is indeed remedied in the measure in use by our author by the flat slider; but it remains in the measure which is still used by other philosophers, which consists only in a common phial not provided with such a slider. The error resulting from the want of this slider may amount to ten, and even more sub-divisions.

VII. The seventh error may depend upon the difference in the diameter of the small measure compared

pared with the diameter of the large one; by which difference it may happen, that the dilation of the air within becomes greater or less by warmth, as the substance of the glass be thicker or thinner, and that the capacity of the tube itself varies for this reason. The difference of the result, however, can be but very small from this cause.

Thus, by computing the number of sub-divisions to which these seven errors may amount, we find them to be 25. But, as all the five measures of airs are let up in the large tube one after another, these errors may, if they were all committed, amount to five times this number, or to 125 sub-divisions.

The mistakes depending from the great tube, or measure, may also amount to seven heads.

I. The first source of error may depend on the inequality in the diameter of this tube, by which a difference of four sub-divisions may easily result in each partition.

II. The second error may depend on the tube not being made rough on the inside, from which a difference of six sub-divisions may happen.

III. The third may be owing to the degree of expansion of the air communicated by the hand in the time this tube is examined to observe the length of the column of air. This difference may amount to four sub-divisions.

IV. The fourth may consist in observing the height of the column of water within the tube, when the water within is not on a level with the water without; by which an

error of three sub-divisions may be committed.

V. The fifth error may depend on the difference of time between the letting up each measure and examining the column. From this head a difference of 10 sub-divisions may ensue.

VI. The sixth may be in determining inaccurately the length of the column of air in the tube, which may amount to five sub-divisions.

VII. The seventh error may depend on the tube being kept in a direction more or less perpendicular, which may amount to three sub-divisions. All those errors resulting from the great tube make together 35, and amount, in the three measures of nitrous air let up in one

experiment, to the number of 105 sub-divisions.

Besides the sources of errors already mentioned, there may happen some others from accidental circumstances, which may be principally three.

I. The degree of heat of the common air may change during the time you make your experiment, and may occasion a greater or less extension of the column of air in the great tube.

II. The weight of the air, or its pressure, may also change in the interval of instituting the experiment.

III. The difference of heat communicated to the tube by the body of the observer himself in the time he is near it to make the observation.

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These three accidental errors, though small in themselves, yet may amount to six or more sub-divisions. So that all the errors already enumerated may amount, if they were all committed together, to 260 sub-divisions.

Besides all the above-mentioned sources of errors that may be committed, either by indifferent instruments, or by want of proper attention, there is one which had always vexed me in former times, and which seemed to me, as well as to many others, almost incorrigible. This is the inconstancy in the quality of the nitrous air, which is found sometimes much stronger or weaker than at other times, though the same method of producing it has been observed.

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Of all the metallic substances, mercury seems to be the best to obtain nitrous air of a constant quantity; but heat must be applied, if a large quantity is required in a short time. I made use, a long while ago, of pin-dust, of which a small quantity, put in diluted spirit of nitre, yields all on a sudden a large quantity of nitrous air of an equal degree of strength: but as the solution is very tumultuous, and a great deal of the pin-dust, together with the nitrous acid, is apt to rush out of the phial, I found it at last better to use common copper. I coil strong copper wire, needed so as to be flexible, up in small curls, and fill the phial with them. Thus the nitrous acid, diluted with five or six times its quantity of water being poured in it, finds a large
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and always about an equal surface of the metal exposed to its action, and yields in a short time a large quantity of nitrous air very constant in quality. Brass seems to me to give nitrous air of a much more inconstant quality. Instead of a glass bottle, I often use an elastic gum bottle, or *caoutchouc*, and, instead of a bended glass tube, I take one made of the same elastic gum. Such tube is easily made by coiling up pieces of caoutchouc bottles in the form of tubes, and sticking them together by their extremities. This wonderful substance possesses a strong power of attraction for itself, so that two pieces cut with a sharp instrument will adhere strongly together, if joined before the cut and smooth edges have been touched by the fingers, or before they are soiled in

in any way. To the extremity of such a tube I adapt a hollow glass stopper of a conical form, so as to fit almost all bottles. A brass ring forced over the neck of the gum bottle presses its substance against the glass stopper, and prevents the nitrous air rushing out.

It is to be observed, that nitrous acid will at last destroy the elastic gum bottles by making its inside brittle, especially if the acid is very concentrated.

Though good nitrous air may be obtained by many ways, yet this air loses gradually its strength, and in a few days, if in contact with water, becomes very much weakened; so that it must be either made new almost every day, or we cannot be sure of the result of the experiment.

The

The method of Abbé Fontana in putting the different species of air to the nitrous test, cut short to the whole difficulty arising from the inconstancy of strength in the nitrous air. By over-saturating the air to be examined with nitrous air, it imports little what strength nitrous air has, though even it had lost almost its whole power of absorbing common air.

In the method adopted by other philosophers, by which always a certain proportion of nitrous air is added, at once to a certain quantity of the air under examination, the result is very uncertain if the nitrous air be not exactly always of the same quality. But in the method of Abbé Fontana this article is of no consequence at all.. The only difference arising from weak
nitrous

nitrous air in this method is, that more measures of it are required before the saturation of the air to be examined is compleated.

The reason of this will appear obvious, if we consider that it is only the true nitrous air which is capable of diminishing respirable airs, and that it performs this diminution in the proportion to its strength, so that weak nitrous air will always diminish common air in the proportion of its own good or bad quality. Now I will suppose that the nitrous air, to be added to the two measures of common air, is become so weak, either by standing, or by the admision of any other air, as to possess only half the strength of good nitrous air. The consequence will be, that as much again of it will be required to saturate

turate the two measures of common air ; and thus, after the saturation of the two measures of common air is compleated, there will remain in the great measure, or tube, a column of air so much the longer as the nitrous air employed was the weaker. I will illustrate it with an example : let us suppose, that after the three measures of strong nitrous air are let up, and the saturation of the two measures of the air under examination be compleated, the remaining column of air be found equivalent to three measures, and eight sub-divisions, or to 308 sub-divisions ; this number, subtracted from the 500 parts or sub-divisions of both airs employed, will give a result of 192, which is exactly the quantity of both airs destroyed. Let us now
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again suppose, that the nitrous air employed was so weak, that, instead of three measures, six were required before the saturation was fully completed, and that thus the remaining column of air in the great tube occupies 608, instead of 308, subdivisions; we shall find that the result will be just the same; that is to say, that, by subtracting the 608 parts remaining from the 800 parts of both airs employed in the experiment, there will be found exactly 192 sub-divisions destroyed; and that thus in both cases the accurate salubrity of the air is ascertained. If such bad nitrous air was only at hand as was just now supposed, it follows, that a longer tube ought to be employed.

This observation, which I owe entirely to Abbé Fontana, is in my
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opinion of the utmost consequence, and throws a great deal of light upon the nature of nitrous air, and upon its wonderful property of destroying respirable air; and it illustrates his ingenious theory of this quality, which, I hope, the author will soon publish, but which I have no right either to claim or to anticipate.

In consequence of this observation we need not be so anxious about the goodness of the nitrous acid, nor about the strength of the nitrous air.

We have now already examined eighteen different heads, from which errors or differences in this important experiment may arise, which may render the whole result of the test fallacious and entirely uncertain.

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There are two other great sources of errors that may be committed in the examination of any respirable air by the nitrous test, which Abbé Fontana has also pointed out and corrected.

The first of these two resides in the act of mixing the two airs. If the nitrous air is let up into the large tube, in which was already the air to be examined, and the tube put by for a while without shaking it, to give time to the two airs to incorporate with one another; or if the two airs are first put in a separate vessel before they are let up into the large tube; there will scarcely ever be two experiments corresponding with one another; the difference will be so great, and the result so uncertain, that it may amount in one experiment to an error of
fifty

fifty subdivisions, which, being added to the above-mentioned 256, will make their number in all 306 subdivisions; the difference of time between the moment of the mixture of both airs and the examination will make also a considerable difference; even a few seconds may make a sensible difference, if the examination be intended to be finished in a little while; but if the two airs after their mixture are left to stand by themselves a long while, as for instance 24 hours, the whole result will be uncertain, particularly if the degree of goodness of dephlogisticated air is to be examined.

The remaining source of error consists in letting up at once all the quantity of nitrous air to be employed in the experiment. The uncertainty of the result arising from

this head is so much the greater when dephlogisticated air is under examination.

I have now pointed out twenty sources of errors, which may be committed in the examination of the salubrity of different airs, which indeed it would be impossible to commit all in one and the same trials, but of which some are continually committed in the methods hitherto in use, and which render the accuracy of the test as uncertain as the experiment itself is: however, I readily acknowledge, that even an accurate philosopher, provided with the same instruments which the Abbé uses, will find it difficult in the beginning to make two experiments with such nicety as not to obtain a difference of some few subdivisions; but he will by a little

little practice soon be convinced, that by this method the degree of salubrity of any air may be ascertained with as much exactness as the degree of heat and cold by the thermometer of Reaumur; for the difference of the result will scarce exceed $\frac{1}{500}$ of the two airs employed, and it would be difficult to judge upon the thermometer of Reaumur of $\frac{1}{6}$ of a degree of his scale.

Though I have for the most part made all my experiments according to the present method of Mr. Fontana, as I was not in possession of a tube long enough to put the various airs to the test, according to the original method of the Rev. Dr. Priestley, with as much accuracy as I could have wished, yet I made a great many trials in a way not essentially different from his, by letting

up one measure of common air with one of nitrous air, and shaking the tube immediately when the two airs came in contact with one another. In examining the dephlogisticated airs, I joined two measures of nitrous airs to one of dephlogisticated air, shaking them in the same way as in the former case; but, as these two measures of nitrous air did not always compleat fully the saturation of this kind of air, I added another measure of nitrous air to it, and so on till no farther diminution took place.

I have been careful to express the two different tests in my experiments chiefly in the first section, so that the reader, who is already initiated in the method of Dr. Priestley, may find how far the result of my experiments

riments will be concordant with the result of his own.

When I had an air worse than common air to put to the test, I found Dr. Priestley's method extremely convenient, by adding one measure of nitrous air to one of the air under examination ; but I always took care to shake the two airs together at the moment of their mutual contact, on purpose to abridge the experiment.

The principal thing to be attended to in putting air to the nitrous test, either by the original method of the Rev. Dr. Priestley, or by any other, is to perform with the greatest nicety every circumstance constantly in the same manner in the various experiments ; for, if you allow the two airs to stand longer one time than another, if you be-

gin to shake them both together sooner or later, if you continue shaking them for a longer or shorter time in one experiment than in another, if you cast up the account by observing the degree of the measure, or of the brass scale, sooner or later, &c. you will find in every experiment very considerable variations.

SECTION II.

Experiments shewing in general the degree of goodness or purity of the dephlogisticated air which the leaves of plants yield in the sun-shine.

Exp. I. TWO handfuls of *grass*, ^{Quantity of} the roots being cut off, were put in ^{the two} an inverted jar holding a gallon, ^{airs destroyed.} filled with pump-water, in the sun between eleven and two o'clock; a great quantity of dephlogisticated air was settled at the inverted bottom of the jar, in which the flame of a wax-taper became very brilliant. By the test of nitrous air, according to the present method of Abbé Fontana, the result was as follows: two measures of it being let up in the
 glaſs.

Quantity of
the two
airs destroyed.

glass tube, and one measure of nitrous air joined to it, the mark stood after shaking and reposing at 1.92 ; a second measure being added, it stood at 1.79 ; after a third measure a 1.68 $\frac{1}{2}$; after a fourth measure at 1.87 $\frac{1}{2}$; after a fifth measure at 415 2.85.

By trying it in the other way familiar to Dr. *Priestley*, the result was as follows : one measure of it, with two measures of nitrous air, occupied 0.88.

By pushing this manner of trial farther, as I do, the result was this : by adding to the two former measures of nitrous air a third one, the mark was 1.83 ; by adding a 319 fourth, it marked 2.81. Thus it appears that by this last method the quantity of both airs destroyed amounts

amounts to 96 sub-divisions less than by *Abbé Fontana's* method.

Quantity of the two airs destroyed.

2. Two handfuls of leaves of a *willow* tree were put in the same way in the sun-shine, between eleven and two o'clock; the dephlogisticated air obtained gave, by the nitrous test of *Abbé Fontana*, the following result:

1.96; 1.83 $\frac{1}{2}$; 1.71; 1.64; 2.55. 445

By the other test it gave the following result: one measure of this air with two of nitrous air occupied 0.85; with three 1.75; with four 2.72.

328

3. Two handfuls of *Lamium Album* were exposed in the same way to the sun-shine, from ten in the morning till two in the afternoon. A great quantity of dephlogisticated air was obtained of a fine quality;

Quantity of the two
airs destroyed.
467

quality; it gave, by Abbé Fontana's test, 1.90; 1.73 $\frac{1}{2}$; 1.53 $\frac{1}{2}$; 1.39; 2.33.

340 One measure of it with two of nitrous air occupied 0.98; with three 1.60; with four 2.60.

4. Two handfuls of leaves of a *vine* were exposed in the same way in the sun-shine, between eleven and one; a good deal of dephlogisticated air was obtained, which gave, by the test,

415 1.92; 1.79; 1.61 $\frac{1}{2}$; 1.87; 2.85.

319 One measure of it with two of nitrous air occupied 0.85; with three 1.83; with four 2.81.

5. One handful of *Becabunga*, the roots being cut off, was exposed in the same way to the open sun-shine, between twelve and four in the afternoon; a great quantity of dephlogisticated air was obtained of

a remarkable fine quality, in which the flame of a wax-taper burned with the most beautiful brilliancy. Quantity of the two airs destroyed.

It gave, by Abbé Fontana's test, $1.87\frac{1}{2}$; 1.73 ; $1.54\frac{1}{2}$; 1.37 ; 2.01 ; 3.00 .

500

One measure of it with two of nitrous air occupied 0.94 ; with three 1.37 ; with four 2.33 .

367

6. A plant of a moderate size of common *thistle*, just before it flowered, was put in the same manner in the sun-shine, from eleven till two o'clock; much dephlogisticated air was obtained of a pure quality; it gave, by Abbé Fontana's test, 1.81 ; 1.51 ; 1.36 ; 1.60 ; 2.60 .

440

One measure of it with two of nitrous air occupied 0.65 ; with three 1.67 ; with four 2.79 .

321

7. Two handfuls of leaves of French *Beans* were in the same way exposed

Quantity of the two
airs destroyed. exposed to the sun during six hours;
a great quantity of very fine dephlog-
gisticated air was obtained, in which

a flame became very brilliant; it
gave, by Abbé Fontana's test, 2.02;
504 1.92; $1.89\frac{1}{2}$; 1.85; 2.01; 2.96.

One measure of it with two of ni-
trous air occupied 0.90; with three
345 1.55.

8. Two little plants of *Teucrium*
Marum were exposed in the same
manner, during four hours, to the
sun-shine; they yielded a great
quantity of dephlogisticated air of a
fine quality; it gave, by Abbé Fon-
tana's test, 1.81; 1.59; 1.37; 1.34;
466 2.34.

One measure of it with two of ni-
trous air occupied 0.60; with three
344 1.59; with four 2.56.

9. Some leaves of *Tobacco* were
in the same manner exposed to the
sun

fun during four hours ; a great ^{Quantity of} deal of dephlogistified air was ob- ^{tity of} tained, which gave, by Abbé Fon- ^{the two} tana's test, 2.07 ; 2.06 ; 2.05 ; 2.41 ; ^{airs de-} 3.39. ^{troyed.}

10. Some leaves of *Cystus Ladani-
nifera*, a very fragrant plant, were
exposed to the sun in the same way
during four hours ; a great quan-
tity of fine dephlogistified air was
obtained ; which gave, by Abbé
Fontana's test, 1.89 ; 1.72 ; 1.56 ;
1.92 ; 2.90.

11. Leaves of *Juniperus Virgi-
niana*, treated in the same way,
yielded a large quantity of fine de-
phlogistified air, whose test was
1.91 ; 1.75 ; 1.60 ; 1.79 ; 2.79.

12. Leaves of *Laurus Camphorata*,
the camphire-tree, treated in the
same way, yielded a good quantity
of very fine dephlogistified air ;
which

Quantity of the two
airs destroyed.
548 which gave, by Abbé Fontana's
test, 2.01; 1.90; 1.78; 1.73;
1.76; 2.56; 3.52.

13. Some branches of *Cedar of Lebanon* were exposed in the same manner, from nine in the morning till two in the afternoon, in the sunshine; a great quantity of dephlogisticated air was obtained; whose test, by Abbé Fontana's method, was 1.95; 1.77; 1.64; 1.51; 2.25; 3.23.
477

14. Some branches of *Artemisia Pontica* were treated in the same way; a great quantity of very fine dephlogisticated air was obtained; whose test, by Abbé Fontana's method, was 2.00; 1.95; 1.85; 1.79; 2.46; 3.46.
454

One measure of it with two of nitrous air occupied 0.92; with three 163.
337

SECTION II.

Experiments, shewing the difference in the purity of the dephlogisticated air, which the leaves of the same plant give at different times.

15. Leaves of an *apple-tree* in an inverted jar full of water were exposed to the sun-shine from ten in the morning till five in the afternoon; the test of the air was, 1.80; 1.58; 1.39; 1.86; 2.79. Quantity of the two airs destroyed. 421

16. Leaves of the same tree exposed during the same time in the sun-shine in a green bottle, 1.82; 1.62; 1.71; 2.64; 3.64. 336

17. The same leaves, which were employed the day before in experiment 15, were exposed again with fresh pump-water in the sun-shine ;

Quan-
tity of
the two
airs de-
stroyed.

556 shine; the air obtained was remark-
ably fine, 1.85; 1.69; 1.54;
1.38; 1.58; 2.49; 3.44.

164 18. Leaves of the same tree ex-
posed to the open air in a very dark
and cloudy day, from five in the
afternoon till six, had yielded but a
small quantity of air, which was
worse than common air, 1.84;
2.36; 3.36.

417 19. Leaves of the same tree ex-
posed in the sun from nine in the
morning till twelve, 1.89; 1.71;
1.52; 1.60; 2.60.

457 20. Air from the leaves of a
willow-tree treated in the same way
as in experiment 1, exposed in the
sun between twelve and two, 2.00;
2.12; 2.19; 2.41; 3.43.

21. Air from the same tree, ga-
thered in a dark, rainy, and windy
day,

day, proved to be of an inferior<sup>Quan-
tity of
the two
airs def-
troyed.</sup> quality, 1.89; 1.71; 1.55; 2.26;
3.26.

374

22. Air from the same leaves gathered between nine and three in a fine warm sun-shine, 1.90; 1.72; 1.53; 2.22; 3.22.

378

N. B. I think the reason of the inferior quality of this air to be, that the leaves were too much crouded in the jar, and that thus too many were deprived from light, being shaded by the others.

23. Air of the same leaves collected in a fine sun-shine between twelve and 5 o'clock, 1.90; 1.71; 1.49; 1.53; 2.52.

467

24. Air of the same leaves gathered in a fine sun-shine between two and five, $1.92\frac{1}{2}$; 1.80; 1.62; 1.60; 2.40; 3.35.

465

O 2

25. Air

Quan-
tity of
the two
airs de-
stroyed.

25. Air of the same, gathered in
a warm sun-shine between three
and five, 1.94; 1.79; 1.63;
401 2.02; 2.99.

SECTION III.

*Experiments tending to investigate
at what time of the day plants
yield the best dephlogisticated air.*

26. THREE jars, each containing
a gallon, were filled with pump-
water, and two handfuls of *willow*
leaves put in each; they were all
exposed at eleven o'clock near one
another in a fair sun-shine.

The

The air of the first jar was examined at half an hour past two in the afternoon; its test was 2.03;

Quantity of the two airs destroyed.

2.05 $\frac{1}{2}$; 2.03 $\frac{1}{2}$; 2.02; 2.54; 3.50. 450

The air of the second jar was examined between four and five o'clock; its test was 2.06; 2.07 $\frac{1}{2}$; 2.06; 2.02; 2.08; 3.03.

497

The air of the third jar was examined between six and seven; its test was 2.12 $\frac{1}{2}$; 2.16 $\frac{1}{2}$; 2.14; 2.12; 2.56; 3.50.

550

27. Three jars, of a gallon each, full of pump-water, were exposed at ten in the morning in the sun, the weather being agreeably warm; each jar contained two handfuls of the leaves of an *Elm-tree*.

The air of the first jar was put to the test at two in the afternoon; the result was 1.90; 1.81; 1.76; 2.66; 3.66.

337

Quan-
tity of
the two
airs def-
troyed.

The air of the second jar was put to the test at four; the result was
377 1.91; 1.77; 1.65; 2.19; 3.23.

The air of the third jar was examined between six and seven; its test gave 1.97; 1.93; 1.85; 2.16;
388 3.12.

28. Two jars, of a gallon each, full of pump-water, were exposed in a very agreeable warm day in the sun at ten in the morning; each contained two handfuls of *willow* leaves.

The air of the first jar was put to the test at three in the afternoon; the result was 1.10; 2.09;
515 2.08; 2.02; 2.06; 2.97; 3.85.

The air of the second jar was examined at five o'clock; it gave by the test 2.09; 2.11; 2.07;
535 2.08; 2.29 $\frac{1}{2}$; 2.78; 3.65.

29. Three

29. Three jars as above were exposed to warm sun-shine between eleven and twelve ; in each were put two handfuls of leaves of an *Elm-tree*. Quantity of the two
airs destroyed.

At three in the afternoon the air of the first jar was put to the test ; it gave $1.91\frac{1}{2}$; 1.93 ; 1.81 ; 2.10 ; 3.10.

390

The second jar was examined at five o'clock, the air gave 1.88 ; 1.67 ; 1.67 ; 2.65 ; 3.65.

335

The third jar was examined at six ; its air gave 1.97 ; 1.88 ; 1.84 ; 2.57 ; 3.54.

346

N. B. On the day this experiment was made it was a very warm sun-shine from eight in the morning till two in the afternoon, when the weather began to be more and more dark ; we had at three a thunder storm ; and after the sky was always

O 4 cloudy,

cloudy, though it continued to be
 warm weather.

Quan-
 tity of
 the two
 airs def-
 troyed.

SECTION IV.

*Experiments tending to investigate
 what quantity of dephlogisticated
 air a certain number of leaves
 yield.*

30. ONE hundred leaves of the
Nasturtium Indicum were put in a
 jar holding a gallon, filled with
 pump-water ; it was exposed to the
 sun in the ordinary way, as in Exp.
 1, between ten and twelve o'clock,
 when so much very fine dephlo-
 gisticated air was settled at the in-
 verted bottom of the jar, that it
 filled

filled a cylindrical jar $4\frac{1}{2}$ inches ^{Quan-} long, and $1\frac{3}{4}$ inch diameter; which ^{tity of} the two ^{airs def-} air gave by the nitrous test 1.94; ^{troyed.} 1.82; 1.67; 1.57; $2.45\frac{1}{2}$; 3.44. 456

31. All the air of these hundred leaves being taken away, they were again exposed to the sun till seven o'clock in the evening, when about half the above-mentioned quantity of dephlogisticated air was again found in the jar, which proved to be still better than the former; its test was 1.99; 1.87; $1.73\frac{1}{2}$; 1.65; $1.93\frac{1}{2}$; 2.85; 3.79. 520

32. After separating again this air from the leaves, I replaced the jar in the open air upon the same place, and left it till next morning at eleven o'clock, when I collected from the same leaves nearly the same quantity as the last time of very fine dephlogisticated air settled at

Quan-
tity of
the two
airs de-
stroyed.

at the top of the jar; it gave by
the nitrous test $1.91\frac{1}{2}$; 1.75 ; 1.58 ;
511 1.44 ; 2.20 ; 3.15 ; 3.89 .

33. Three leaves of a favoy-cab-
bage, of a moderate size, were put
in a similar jar with pump-water,
in a fair sun-shine, between twelve
and two o'clock, when two ounce-
measures of dephlogistified air were
disengaged: which gave by the ni-
trous test 1.94 ; $1.78\frac{1}{2}$; 1.63 ;
364 2.38 ; 3.36 .

34. A handful of the leaves of
Nasturtium Indicum yielded, in a
few hours on another day in a fine
sun-shine, one ounce-measure of
dephlogistified air, of the follow-
ing goodness, 1.93 ; 1.76 ; $1.56\frac{1}{2}$;
504 1.39 ; 1.99 ; 2.96 .

35. Seven and twenty single
leaves of a walnut-tree were in the
same manner exposed to the sun-
shine

shine in a fair warm day, from eleven till five o'clock, when they had yielded about one ounce-measure of good dephlogisticated air.

SECTION V.

Experiments tending to investigate the quality of air yielded by plants in the night, and by day in dark or shaded places.

36. TWO handfuls of grafs, the roots being cut off, were put in an inverted jar of a gallon, full of pump-water, and placed in a room during the night, the jar being covered so that no light could come at it before I examined it. In the morning

morning a small quantity of air was settled at the inverted bottom of the jar, in which a candle was extinguished directly.

37. At nine o'clock in the evening, August 8, when no leaves would yield any more air bubbles, except potatoe-leaves (which always begin the first to yield air in the morning, and cease to yield them at night the last of all), I filled different jars with pump-water, and put in each a good deal of leaves of some or other plant or tree, viz. leaves of *oak*, *lime-tree*, *willow*, *yew-tree* or *taxis*, *apple-tree*, *sage* or *salvia*, *artichoke*, *persicaria urens* or *water pepper*, *potatoe* leaves : they were all kept in a room till next morning, when I examined the air which they had yielded.

Early

Early in the morning I found they all had yielded but an inconsiderable quantity of air.

The air of the oak leaves and walnuts was the worst of all, it was not diminished by nitrous air; that of the willow and the fage was near as poisonous; that of the lime-tree was not quite so bad; that of the artichoke was somewhat better; that of the potatoe-leaves was the least poisonous of all; however, in none of them a candle would burn even for a moment. That of the *water-pepper* was so poisonous, that it extinguished the flame, though diluted with five times as much common air; the apple-tree leaves had yielded so little air that I could not put it to any test.

38. Two handfuls of leaves of *French beans* were put in a jar full of
of

of water, which was kept inverted the whole night in a room; next morning a small quantity of air was obtained, which proved to be of a very poisonous quality; it extinguished directly a flame. One measure of it with one of nitrous air occupied 1.94; so that an animal absolutely could not have lived in it during one single minute.

SECTION VI.

Experiments tending to investigate to what degree plants may affect common air in the night, and by day time in shaded places.

39. SOME plants of grafs, with roots and all, were put in a jar of a gallon; the jar was inverted upon a dish, and some water put in the dish to keep the plants alive, and to cut off the communication with the external air; in the morning the air in the jar was altered for the worse, the flame of a wax-taper grew dim in it. One measure of it with one of nitrous air occupied 1.24.

40. Two handfuls of leaves of *French beans* were put in a jar of a gallon ; it was kept inverted upon a dish, and some water poured upon it ; next morning I found the air so much fouled that a candle could not burn in it. One measure of it with one of nitrous air occupied 1.39.

41. After having taken out some of the air for trial, I placed the jar with the remaining air and leaves in the sun from nine till eleven o'clock, when I found the air so much mended, that a candle could burn in it, and that one measure of it with one of nitrous air occupied 1.12.

After this I replaced it again in the sun till five in the afternoon, when I found the air so much mended as to be equal in goodness to common air.

42. A little branch of *Cedar of Lebanon* was placed over-night in a room under a cylindrical jar of about two ounce-measures; next morning the air was much fouled by it. One measure of it with one of nitrous air occupied 1.45.

43. Three jars inverted were placed upon dishes in the manner as in Exp. 39; under one was placed a potatoe-plant; under the second a plant of common *nightshade*; under the third a plant of *hyoscyamus*.

Next morning I found the air of all three very much fouled, but that of the *hyoscyamus* the most of all. In none of them would a candle burn. One measure of the air of the potatoe-plant with one of nitrous air occupied 1.59; one measure of that of the *nightshade* occupied with

P

one

one of nitrous air 1.77; and that of the *hyoscyamus* 1.83.

44. I placed a very lively chicken, eight days old, under a jar containing a quart full of the air fouled by the *hyoscyamus* plant in the preceding experiment; it directly grew sick, and was ready to expire in less than half a minute. I took it immediately out of the jar, and put it directly in another jar full of dephlogisticated air drawn from the leaves of plants. The creature remained for some minutes quite motionless, shewing no signs of life but by drawing languidly its breath; it recovered gradually in this air in six or seven minutes so much that it began to stand upon its legs and to jelp now and then with a very weak voice. I then took it out of the dephlogisticated air and put it
upon

upon my hand in the open air ; as soon as it came out of the last jar it seemed to grow worse, and could stand no more : however it recovered gradually again.

45. A sprig of *pepper-mint* was placed under an inverted jar upon a dish, with some water upon it, to keep the plant alive, and to shut off the communication with the air in the jar, and the air without. It was placed in a warm day in a room against the window towards the sun, from eleven till one o'clock. Another sprig of the same plant, and of the same size, was put under a similar jar, and placed upon the floor far from the windows in a room very well lighted, but in which the sun did not shine at that time. By examining both airs I found the air of the jar which was

placed towards the sun rather better than the common air was at that time. But the air of the other jar was less good than the common air; for one measure of it with one of nitrous air occupied 1.13; whereas one measure of the common air with one of nitrous air occupied $1.06\frac{1}{2}$.

46. Two handfuls of leaves of a *walnut-tree* were put into a gallon jar, filled with pump water, and placed about four feet from the wall of the house towards the north, and under the shade of raspberry-bushes, so that no rays of the sun could reach it. After standing there during seven hours in an agreeable warm day, scarce $\frac{1}{10}$ of air was obtained of what these leaves used to give in the sun-shine; and this air was of such a bad quality that a candle

candle could not burn in it : where-^{Quan-}
as two handfuls of walnut-leaves^{tity of}
placed in the sun-shine had yielded^{the two}
in the same time a great quantity^{airs def-}
of very pure dephlogisticated air,^{troyed.}
whose test was 1.69 ; 1.82 ; 1.69 ;
1.54 ; 2.35 ; 3.34.

466

47. At the same time two handfuls of *oak* leaves were in the same manner placed under the same bushes ; the air of these leaves was remarkably foul, for it was very little diminished by nitrous air.

48. The same was done with *willow* leaves ; the air obtained was also of a bad quality, but better than that of oak or walnut-leaves.

49. The same was also done with leaves of *elm-tree* ; the air obtained was very foul, as one measure of it with one of nitrous air occupied 1.90.

50. I placed over-night in a room about an equal quantity of branches of a lime-tree, walnut-tree, a vine, an oak, and a willow, each in a different jar holding a gallon, without water, putting only some water in the plate upon which the jars were inverted, to keep the branches alive; the jars were left without being covered, and examined between eight and nine in the morning; the air of all was tried by putting one measure of it with one of nitrous air immediately one after another in the glass tube.

The air of the lime-tree occupied	- - - -	I.24
the air of the walnut-tree		I.25
_____	vine	I.30
_____	oak	I.26
_____	willow	I.23

51. After

51. After having taken out as much air as was wanted for the trial, the jars were placed in the garden in a fine sunshine, and the air of them was again examined between ten and eleven.

The air of the lime-tree occupied - - - - 1.08

the air of the walnut-tree 1.07 $\frac{1}{2}$

————— vine 1.05 $\frac{1}{2}$

————— oak 1.12 $\frac{1}{2}$

————— willow 1.07

After which they were again exposed to the sun till three in the afternoon, and their air examined.

The air of the lime-tree occupied - - - - 1.06

the air of the walnut 1.05

————— vine 1.05 $\frac{1}{2}$

————— oak 1.12 $\frac{1}{2}$

————— willow 1.07

So that the same air which the plants had fouled in the night was again restored to its former purity, and even by some of these plants rendered better than common air, principally by the walnut-leaves, lime-tree, and vine, the test of the common air being at the time that one measure of it with one of nitrous air occupied 1.07.

SECTION VII.

Experiments shewing that the damage done to common air by the night is very inconsiderable compared to the benefit it receives in the day.

52. TWO handfuls of *Elm-tree* ^{Quan-} leaves were put in a jar full of wa- ^{tity of} ter, as in the former experiment, ^{the two} and exposed to the open air from ^{airs def-} the 14th of August to the 16th, ^{stroyed.} when the air disengaged from them was examined, and found to be good dephlogisticated air; its test was 1.95; 1.85; 1.77; 2.40; 3.50. 350

53. Some grafs, kept in a green bottle from the evening till next day at eleven o'clock, had yielded some air, whose test was 1.80; 1.64; 1.54; 2.31; 3.26. 374

54. A

Quan-
tity of
the two
airs def-
troyed.

54. A handful of *Persicaria urens*, water pepper, was kept in a green bottle, and exposed to the open air from the evening till next day at ten o'clock, when the air collected from it was found of the following
226 test, 1.90; 1.86; 2.78; 3.74.

55. Two handfuls of leaves of *Lauro-cerasus* were put in a jar full of water, and exposed to the open air during 24 hours; a good quantity of dephlogisticated air was obtained, whose test was 1.78; 1.61;
306 2.02; 2.97; 3.94.

SECTION VIII.

Experiments shewing that plants have a remarkable power to correct bad air in the day.

56. A sprig of *pepper-mint* put in a jar full of air fouled by breathing (so as to extinguish a candle), and exposed to the sun, had corrected this air in three hours so far that a candle could burn in it.

Quantity of the two airs destroyed.

57. A sprig of a *nettle* was put in a jar full of air fouled by breathing so as to extinguish a candle ; it was placed in a room during the whole night ; next morning the air was found as bad as before. The jar was put at nine in the morning in the sun-shine ; in the space of two

Quantity of the two
airs destroyed. two hours the air was so much corrected, that it was found to be nearly as good as common air.

58. A sprig of *Perficaria urens* was put in a phial full of air fouled by breathing, so as not to allow a candle to burn in it ; it was exposed to the sun during an hour and an half, in which time the air was so much corrected that a candle could burn in it. The same effect was obtained from a sprig of a vine, and that of a camomile plant, and from some rushes.

59. A *mustard* plant was put in a jar; the stem was cut off on a level with the orifice of the jar ; the jar was then inverted in an earthen pan containing some water to keep the plant alive, and placed over-night in a room ; next morning the air of the jar was found much fouled,
so

so as to extinguish the flame of a ^{Quan-}
wax-taper ; its test was, by Abbé ^{tity of}
Fontana's method, 1.98 ; 2.87 ; ^{the two}
3.83. ^{airs def-} 117
^{troyed.}

60. It was then exposed to the
sun, and examined again after a
quarter of an hour, and found al-
ready somewhat corrected ; for its
test was then 1.97 ; 2.84 ; 3.79. 121

The jar was again put in the
open air, when, after standing $1\frac{1}{2}$
hour in the sun, the air was found
to be remarkably corrected ; for
now its test was 2.01 ; 2.25 ; 3.24. 176

The jar was again replaced in
the sun ; when it had been exposed
during three complete hours, the
air was so much improved as to be
better than the common air at the
time ; for its test was 1.95 ; $2.21\frac{1}{2}$;
2.20 180

The

Quantity of
the two
airs destroyed.

The test of the common air was at that time, by Abbé Fontana's method, 1.96 ; 2.25 ; $3.26\frac{1}{2}$.

See also Experiments 40 and 41.

SECTION IX.

Experiments shewing that acrid, stinking, and poisonous plants yield by day-time as good dephlogisticated air as any others.

61 A plant of *Hyoscyamus* was exposed in the ordinary way in a jar full of water in the sun-shine, from twelve till five o'clock ; a great deal of fine dephlogisticated air was obtained, in which the flame of a wax-taper was very brilliant. One

measure of it with two of nitrous air occupied 0.93 ; with three 1.70. Quantity of the two airs destroyed.

62. Leaves of *Lauro-cerasus* (one of the most active poisons that exists in the vegetable kingdom, when in a concentrated state, killing an animal almost in an instant, and equally poisonous when taken in the stomach, as when applied to a wound, which last effect was lately discovered by Abbé Fontana) treated in the ordinary way yield a good deal of dephlogisticated air. Two handfuls of them exposed in water to the sun, between eleven in the morning and five in the afternoon, yielded a good deal of dephlogisticated air, whose test was 1.87 : 1.67 ; 1.50 ; 2.04 ; 3.04.

394

63. Two handfuls of common *night-shade*, a suspected plant, exposed

Quantity of the two
airs destroyed. posed in the sun between two and five o'clock, had yielded a great deal of dephlogisticated air, whose test was $1.92\frac{1}{2}$; 1.79 ; 1.65 ; 1.52 ; 495 $2.08\frac{1}{2}$; 3.05 .

64. I got in the same way good dephlogisticated air from *tobacco* leaves, (see Exp. 9); from *Atriplex Vulvaria*, a plant of a very particular offensive smell; *Cicuta Virosa* or *water hemlock*, one of the most dangerous poisons; and *Sabina*.

SECTION X.

Experiments shewing that all flowers in general yield a very poisonous air, though in a very small quantity, and are apt to spoil a great quantity of good air by day and by night.

65. TWO handfuls of flowers of *Marigold* or *Calendula* were exposed in pump-water to the open air during 48 hours ; a small quantity of air was obtained, which extinguished flame directly, and was scarcely diminished at all by nitrous air.

66. Two handfuls of *Camomile* flowers were put in a quart jar filled with pump-water, and inverted :
Q ed :

ed: after two days standing in the garden, a small quantity of air was obtained, in which a flame was immediately extinguished.

67. Forty-five flowers of *Mari-gold* were put in a quart jar without water, and kept the whole night in a room, the jar being inverted; next morning I found the air so much fouled by them that a candle would not burn in it. One measure of it with one of nitrous air occupied 1.43.

68. I placed the same flowers with the remaining air in the sunshine from nine till twelve, when I found the air still more infected. One measure of it with one of nitrous air occupied 1.54.

69. A few flowers of *Honey-suckles*, *Caprifolium*, were placed under a jar of about a pint; when they had

had stood about three hours in the room, the air of the jar was so much infected that a candle could not burn in it.

The same effect was obtained with a similar quantity of the same flowers exposed during three hours in the sun-shine.

70. A similar quantity of these sweet-scented flowers were kept in a pint jar over-night; when they had so much fouled the air that an animal must have died in it, one measure of it and one of nitrous air occupied 1.68.

All kinds of flowers had nearly the same effect. All of them fouled the air more or less, either in a room or in the open air, as well by day as in the night, equally in sun-shine as in a shaded place.

SECTION XI.

Experiments shewing that roots of plants when kept out of the ground yield, in general, bad air, and spoil common air at all times, some few excepted.

Quan-
tity of
the two
airs de-
stroyed.

71. THREE handfuls of roots of mustard plants, washed clean, were put in a jar full of water in the ordinary way, and exposed to the sun during six hours; when some air was obtained, which extinguished a candle directly.

72. Two handfuls of roots of common *rusks*, well cleaned from dirt, were in the same manner exposed to the sun during seven hours; when a small quantity of air was
6 obtained,

obtained, in which a candle could not burn.

Quantity of
the two
airs destroyed.

73. One handful of roots of mustard plants, cleanly washed, was put in a quart jar full of water, and three ounce-measures of common air let up : after six hours standing in the sun, the air was found changed for the worse, for its test was, by Abbé Fontana's method, 1.95 ; 2.34 ; 3.37.

162

74. A handful of roots of *Becabunga* was exposed to the sun in a quart jar filled with water during six hours ; a moderate quantity of air was obtained, which, by the nitrous test, proved to be as good as common air.

All other roots which I tried yielded bad air, and spoiled ordinary air at all times.

SECTION XII.

Experiments shewing that all fruits in general yield bad air, and infect ordinary air at all times, but principally in a dark place, and in the night.

75. Six *peaches* of a small size were put under an inverted quart jar placed upon a dish, in a room not very light, between two in the afternoon and seven in the evening; when I found the air in the jar so much spoiled, that a candle could not burn, nor an animal live in it. One measure of it with one of nitrous air occupied 1.86.

76. Two of these *peaches*, being put under the same quart jar during
two

two hours, had so much altered the air that a wax-taper could scarcely burn in it a moment, but was ready to go out.

77. Six *peaches* of a small size were placed under an inverted quart jar in the sun between nine and eleven ; when, by examining the air, I found it to extinguish a candle. One measure of it with one of nitrous air occupied 1.55.

78. One *lemon* placed under a jar, containing three-quarters of a pint, infected the air so much in a few hours, that a candle burned dim in it.

79. One handful of *filberts* were placed under a jar of two pints during the night ; I found the air in the jar so much fouled as to extinguish a candle.

Q 4 80. Six

80. Six small *Bergamot* pears were put over-night under a jar of two pints; the air was altered somewhat for the worse; a flame grew dim in it. One measure of this air with one of nitrous air occupied 1.25.

81. Three apples nearly ripe were placed under a two-pint jar over-night; the air was found much infected by them; it extinguished a flame. One measure of it with one of nitrous air occupied 1.48.

82. The remainder of the air in this jar was kept with the apples, and exposed to the sun during seven hours, when the air was become still worse. One measure of it with one of nitrous air occupied 1.72. A flame was directly extinguished in it.

83. Four

83. Four lemons were placed under a quart jar in the sun during seven hours, when the air was changed for the worse. One measure of it with one of nitrous air occupied 1.18.

84. A jar holding a gallon was one-third filled with ripe *mulberries*, and exposed to the sun, being inverted upon a dish; in the space of four hours the air in the jar was so much infected as to extinguish a flame directly. One measure of it with one of nitrous air occupied 1.63.

85. *Plumbs* and *blackberries*, ripe and unripe, spoiled also common air in the sun and in the dark.

86. Six apples, as soon as taken from the tree, were directly put in a gallon jar full of pump-water, and exposed to the sun, the jar being
inverted

inverted upon a dish; the apples became covered with a great number of small air-bubbles. After they had been thus exposed to the sun from ten in the morning till four in the afternoon, a moderate quantity of air was obtained, which proved to be very bad; a flame was directly extinguished in it. One measure of it with one of nitrous air occupied 1.69.

87. Two dozen of young and small *French beans* were put in a quart jar full of water, and exposed in the sun from ten till two o'clock: they were covered all over with a great many small air-bubbles; the quantity of air collected was but small, and in quality somewhat worse than common air. One measure of it with one of nitrous air occupied 1.14; whereas one measure

sure of common air with one of nitrous air occupied $1.08\frac{1}{2}$.

88. Two dozen of young and small French beans were put under an inverted quart jar overnight in a room without water; they stood till eleven in the morning and were not covered, so that they had been a long while exposed to all the light of the room. The air in the jar was found so remarkably poisoned, that it even surpassed in foulness the air infected by a plant of *Hyoscyamus* (see Exp. 43); for one measure of it with one of nitrous air occupied 1.95.

89. I was willing to see the effect of such offensive air upon a living animal. I placed a very lively chicken eight or nine days old in this air; in the very instant it entered the jar it shewed signs of the utmost

utmost anxiety, fell down motionless, and died in less than half a minute. When I saw it dying, I took it out with all the expedition possible, in order to recover it in another jar full of dephlogisticated air, which I had kept ready for the purpose; but, notwithstanding it had not been 20 seconds in this foul air, it was quite deprived of life.

Comparing the suddenness of destroying the life of an animal with this air, with that of inflammable air drawn from metals by vitriolic or marine acid, I found that the air fouled by these beans was as destructive to animal life as the inflammable air itself.

90. I placed six of these beans over-night in a gallon jar inverted upon a plate, on purpose to see whether

whether such a small number of them could affect observably such a great body of air. I was astonished to find they had so much affected the air, that it was rendered quite unwholesome for breathing; it extinguished a flame; and one measure of it with one of nitrous air occupied 1.34.

91. Three small unripe walnuts were put under a jar of about three ounce-measures, from twelve till two o'clock, in a room by day, when the air in the jar was so much spoiled as to extinguish flame. Its test was, that one measure of it with one of nitrous air occupied 1.54.

SECTION XIII.

Experiments shewing that no part of plants improve ordinary air, or yield dephlogisticated air, but the leaves and the green stalks.

Quan-
tity of
the two
airs de-
stroyed.

92. THE former experiments with flowers, roots, and fruits, are already above related. There remain only the green stalks or branches, not yet covered with the rough skin or bark, and the wood itself, to be examined.

I put some green stalks of a willow-tree, the leaves being stripped off, in a gallon jar filled with pump-water ; the jar was exposed, inverted, as ordinary, upon a wall in a warm sun-shine during four hours.

They became most beautifully covered with an infinite number of round air-bubbles. A great deal of dephlogisticated air was obtained, which gave, by the nitrous test, 1.96; 1.87; $1.83\frac{1}{2}$; 2.68; 3.64. 336

93. Some branches of a mulberry-tree, covered with grey bark, were put in a gallon jar full of pump-water, and exposed to the sun. A moderate quantity of air was obtained, which, being put to the nitrous test, proved to be about the same quality with common air; its test was 2.01; 2.10; 3.10. 190

SECTION XIV.

Experiments shewing what kind of water obstructs least the natural operation of leaves yielding de-phlogisticated air.

94. AN equal number of willow leaves were put in four different jars, each holding a gallon ; one jar was filled with stagnating water taken out of a pond rather unclean ; the second jar was filled with rain-water collected the day before ; the third with river-water ; and the fourth with water taken fresh from the pump. They were all placed, at eleven o'clock, upon a wall in the sun-shine ; and the air yielded by the
the

the leaves was taken out of the jars at three in the afternoon.

Quantity of
the two
airs de-
stroyed.

The result was, that the leaves put in pond-water had yielded the least quantity of air, and that of no better quality than common air. Those in the rain-water had yielded more air, and of a better quality. Those in the river-water had yielded still more and better. The pump-water had yielded the most and best of all.

To be able to judge the better of the exact degree of purity of those airs, I put them all to the nitrous test; the result was as follows :

Air from the leaves in stagnating-water 2.04 ; 2.20 ; 3.22. 178

Air from the leaves in rain-water 1.94 ; 1.96 $\frac{1}{2}$; 2.69 ; 3.69. 231

R

Air

Quantity of
the two
airs destroyed.

[242]

256 Air from the leaves in river-water
2.05 ; 2.04 ; 2.47 ; 3.44.

Air from the leaves in pump-
water 1.96 ; 1.85 ; 1.72 ; 1.64 ;
456 2.47 ; 3.44.

95. I put a handful of leaves of
a willow in a jar full of newly
distilled water, and exposed it to
the sun during four hours ; the
leaves gathered some bubbles upon
the under side, but very few upon
the upper side ; and very little air
was obtained, scarcely enough to put
it to the test ; and of this air about
 $\frac{1}{5}$ was common air, which had slipped
in by inverting the jar. It was very
far from being dephlogisticated air ;
it was even worse than common
air.

96. I obtained some water which
was distilled some months ago, and
put some leaves of a vine in it. A
small

small number of bubbles settled upon the under side of the leaves, but very few upon the upper surface. The jar was placed in the open air during about five hours, the weather being cloudy. A small quantity of air was obtained, which was worse than common air.

97. I impregnated some water drawn out of a well with fixed air by Dr. Noot's contrivance, or by the glass apparatus sold at Mr. Parker's. I put some leaves of a vine in a jar full of this water; as soon as they were under water, they were all covered most beautifully with bubbles. After standing about five hours in the garden in a cloudy day, some air was obtained, which proved also, by the nitrous test, to be worse than common air, the greatest part of it being absorbed

R 2

by

by the water before it was put to the test.

98. I impregnated a jar full of water with fixed air by means of salt of tartar and spirit of vitriol, according to the method of Dr. Hulme. I put some leaves of a vine in this water, which I found covered with air-bubbles as soon as they were plunged under the water, first at the under surface, and soon after at the upper surface also. After standing about four hours in a warm sun-shine, I found a very large quantity of air collected at the inverted bottom of the jar, which I found by far the greatest part to be fixed air, as it was absorbed in the water by shaking. I put to the nitrous test that part of it which remained unabsorbed, and

and found it inferior in quality to common air.

It might be found reasonable to think, that those numerous air-bubbles, which appear upon the leaves as soon as put under the surface of the water impregnated with fixed air, are owing to the fixed air settling in the form of these bubbles upon the surface of the leaves.

The sudden appearance and increase of these bubbles depend greatly upon the fixed air settling on the surface of the leaves; for any other body gets also bubbles in such water; but the vital motion of the leaves acts its part in this scene; for these bubbles appear first on the same surface of the leaves on which they appear in common water. It appeared, by a variety of experiments I made on this head, that

water much impregnated with fixed air disturbs the natural operation of the leaves in yielding dephlogistified air, and that the air thus obtained was chiefly the fixed air from the water, and some little quantity of air, which is sometimes better than common air, but for the most part much worse.

SECTION XV.

Experiments shewing to what degree of purity dephlogisticated air may be elaborated by vegetables.

IT has appeared in the course of several hundred experiments which I made in my retirement, that leaves of plants in general yield the finest air when they are not much crowded together, so that the most part of them receive the direct influence of the sun principally in the afternoon between mid-day and six o'clock in the middle of the summer.

99. I obtained from several plants such a pure dephlogisticated air, that the flame of a wax-taper not

R 4 only

Quan-
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the two
airs de-
stroyed.

only burned in it with such a degree of brightness that it dazzled my eyes, but it excited a crackling hissing which accompanies the flame when plunged in pure dephlogisticated air. Among the plants which yielded the purest airs were some aquatic plants and the turpentine-trees, from which I always got air of an eminent degree of purity, so that sometimes six measures of nitrous air were required before the saturation of the two measures of the dephlogisticated air could be obtained, and that above $\frac{500}{800}$ of the bulk of the two airs were destroyed.

100. In September I got from young leaves of a vine such pure air that its test gave the following result: 1.97; 1.87 $\frac{1}{2}$; 1.78; 1.68; 2.33; 3.30.

101. And

101. And the same day, from full-grown leaves of a vine still purer, it gave the following result:

Quantity of
the two
airs destroyed.

1.95; 1.85; 1.72; 1.60; 1.61;

2.53.

547

102. The air obtained from the green matter surpassed in purity the dephlogisticated air obtained from leaves; this purity was so great, that this dephlogisticated air required eight measures of nitrous air to saturate two measures of it, and that $\frac{645}{1000}$ of the bulk of the two airs were destroyed. The dephlogisticated air, which I obtained from the green matter collected from a stone-trough kept full of water near a spring upon the high-road, was so great, that $\frac{652}{1000}$ of the bulk of the two airs were destroyed before the complete saturation was obtained.

As

Quantity of
the two
airs de-
stroyed.

As this green matter is probably of the vegetable kind, I make no doubt but as good dephlogisticated air might be obtained from leaves of plants by some way or other which I have not yet been lucky enough to hit upon.

However pure this dephlogisticated air may be, that which may be extracted from certain substances which do not belong to the vegetable kingdom is still superior to it, as is the air obtained from nitre and red precipitate.

To give an exact account of the nature of these airs, I will place here the result of the first mentioned dephlogisticated air, drawn from the green matter, produced by itself in the jar. The method of trying it was that of Abbé Fontana, 2.05; 2.01; 1.93; $1.81\frac{1}{2}$; $1.72\frac{1}{2}$; $1.70\frac{1}{2}$; 645 $2.62\frac{1}{2}$; 3.55.

The

The result of the test of the de-^{Quan-}
phlogistified air, obtained from the^{tity of}
green matter gathered from the^{the two}
stone-trough on the high-road,^{airs des-}
2.08 ; 1.07 ; 2.01 ; 1.92 ; 1.89 ;
1.78 ; 2.54 ; 3.48. 652

How near the purity of this air
approaches to that of the dephlo-
gistified air, extracted by fire from
red precipitate, may be seen in the
following test of it : 1.63 ; 1.28 ;
93 ; 59 ; 27 ; 58 ; $1.02\frac{1}{2}$; 2.50. 750
So that the two measures of this
dephlogistified air had been re-
duced to about $\frac{1}{7}$, and that $\frac{750}{1000}$ of
both airs had been destroyed to
complete the saturation.

SECTION XVI.

Experiments shewing the effect of plants upon inflammable air.

103. TWO ounce-measures of inflammable air (which was so strong as not to be diminished at all by nitrous air) were let up in a quart jar containing one handful of pepper-mint sprigs; it stood over-night within the house: next day I found the bulk of the air somewhat increased, but still so bad as not to be diminished at all by nitrous air.

104. The same evening I put two ounce-measures of inflammable air in a similar jar with one handful of walnut-leaves; next day
I found

I found the bulk of air increased to about $\frac{1}{10}$. One measure of it with one of nitrous air occupied 1.90.

105. Two ounce-measures of inflammable air was also let up in a similar jar with one handful of *Persicaria urens* or water-pepper; next day I found the bulk of the air diminished about $\frac{1}{25}$. One measure of it with one of nitrous air occupied 1.97.

N. B. All these three jars stood in the house from the evening till between twelve and one next day, so that the light of the day may have extricated some air from the pepper-mint and the walnut-leaves.

As neither of these plants could be said to have really corrected this poisonous air, I was curious to see what

what effect they would have upon the same air in the fun.

Exp. 106. For this purpose I let up again two ounce-measures of the same inflammable air in the jar containing the walnut-leaves Exp. 104; and placed it in the fun between two and five o'clock; when I found the bulk of the air increased to $\frac{1}{4}$, but very little corrected, for one measure of it with one of nitrous air occupied 1.89. See, in Exp. 107, the reason why these leaves failed to correct this air.

Exp. 107. I also let up two ounce-measures of the same inflammable air in the jar containing the *Perficaria urens* Exp. 105; and placed it in the fun between two and six; when I found the bulk of the air increased $\frac{1}{12}$, and so much mended, that one measure

of

of it with one of nitrous air occupied 1.33.

108. I had also let up, in the same jar in which the pepper-mint had been the whole night, two measures of inflammable air, and kept it in the sun about three hours; but, having forgot to copy the result of this experiment in my notes, I repeated it next day by itself. I let up two ounce-measures of inflammable air in a quart jar, in which I had put four sprigs of pepper-mint, so that the whole made up about one handful; I placed it in a fine sun-shine from one till half past four; when I examined the air, I found it increased about $\frac{1}{10}$, and very much mended; for one measure of it with one of nitrous air occupied 1.21; so that it approached very much to the nature

of respirable air ; it exploded however with a loud report.

109. As, in experiment 106, the two measures of inflammable air let up in the jar containing the walnut-leaves were scarce corrected at all in the day time, whereas the other plants had corrected this air in a very great measure ; I suspected that the walnut-leaves had suffered from the inflammable air in the night time, and that, perhaps, they had lost their natural power of correcting this kind of air, or that some mistake had been committed ; I thought it therefore advisable to repeat the experiment another day, which I did. Having let up in a jar filled with pump-water, in which a handful of walnut-leaves were, two measures of inflammable air, and left the

the jar in the sun-shine from twelve till five o'clock, I found the air much corrected, for one measure of it with one of nitrous air occupied 1.30. The air was very explosive.

I was now satisfied that all plants possess the power of correcting inflammable air; but I wanted to see whether plants could reduce inflammable air to the purity of common air, by letting the inflammable air remain during two or more days with the plant.

110. A measure of inflammable air was let up in a jar containing a handful of *Persicaria urens*, and another measure in a jar containing a handful of leaves of a walnut-tree. They stood 48 hours in the open air, when I examined them.

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 the two
 airs de-
 stroyed.

The inflammable air put with the walnut-leaves seemed to be corrected so much as to appear, by the nitrous test, better than the common air was at the time; for one measure of it with one of nitrous air occupied 1.03; whereas one measure of common air with one of nitrous air occupied 1.05. This inflammable air gave the following result, by Abbé Fontana's test, 1.91; 2.16 $\frac{1}{2}$; 3.16.

Having filled a cylindrical jar with this air, I found it explode with an uncommon loud report, which surprized me not a little, and gave me some apprehension that the nitrous test might fail in some instances. The inflammable air let up in the jar with the *Perficaria urens* gave the following test: one measure of it with one of nitrous

nitrous air occupied 0.95; and ^{Quantity of} with two measures of nitrous air ^{the two} 1.92. By Abbé Fontana's method ^{airs destroyed.} it gave 1.90; 1.96; 2.95. 205

Thus this air seemed to surpass far the goodness of common air.

III. I then tried it by the flame of a candle, and found it to explode with a very loud report. As I thought the result of these trials very extraordinary, and to afford a remarkable exception in the application of nitrous air to the test of any air, I repeated each of these experiments twice, and obtained constantly the same result.

II2. I was resolved, however, to repeat again the experiment: Some plants of *Persicaria urens* were put in a gallon jar, and a good quantity of pure air was let up in the jar. It was kept in the open air from Sunday till Friday follow-

ing, when it was examined, and found to be so poisonous that a chicken, three weeks old, died in it in less than a minute. It proved also very bad by the nitrous test; for one measure of it with one of nitrous air occupied 1.80; and the result of Abbé Fontana's method was 2.58; 3.58.

This result, being quite different from the result in Experiments 108, 109, 110, and 111, restored my hope that some blunder had been committed in the experiments just mentioned; I resolved therefore, if possible, to discover this mystery.

113. Two pints of strong inflammable air (which could not be diminished by nitrous air) were let up in a gallon jar containing some plants, with roots and all, of *Persicaria urens*, which was placed in

3

the garden. After it had stood 24 ^{Quantity of} hours, the air was examined, and ^{the two} found much mended; for one ^{airs destroyed.} measure of it with one of nitrous air occupied 1.23; it exploded with a loud report. It was again replaced in the garden, and examined after it had stood 48 hours, when it was found, by the nitrous test, at one o'clock in the afternoon, near as good as common air; for one measure of it with one of nitrous air occupied 1.11½. It gave, by Abbé Fontana's test, 2.04; 2.33½; 3.32. And yet it still exploded as before. 168

After this trial it was again placed in the open air, and re-examined the same day between four and five in the afternoon, when the nitrous test indicated it to be better than common air; for one measure

of it with one of nitrous air occupied $1.06\frac{1}{2}$; whereas one measure of common air and one of nitrous air occupied at that time 1.08.

114. This result convinced me entirely that the nitrous test really fails in this kind of air; for though it gave all the appearance of good air, yet it exploded with a loud report; and a chicken placed in it grew immediately sick, and was ready to expire in six minutes, when I took it out quite motionless.

115. The remainder of the same inflammable air, which had stood during six days with the *Persicaria urens* in Exp. 112, without being much changed, was let up in a jar containing a plant of mustard. After standing 24 hours in the garden I put it to the test, when I found it so much mended, that one measure
of

of it with one of nitrous air occupied 1.02 ; one measure of it with two of nitrous air occupied 2.00. <sup>Quantity of the two
airs destroyed.</sup>

The result of Abbé Fontana's test was 1.96 ; 2.13 $\frac{1}{2}$; 3.12 $\frac{1}{2}$. 187 $\frac{1}{2}$

So that it already surpassed in appearance the best common air ; it exploded however with a loud report. I placed the jar again in the garden, and examined the air after it had stood during 48 hours, when I found it to all appearance still more improved ; for one measure of it with one of nitrous air occupied 0.96 ; and with two measures of nitrous air 1.80. The result of Abbé Fontana's test was 1.97 ; 1.93 ; 2.72 $\frac{1}{2}$; 3.66. 235

It still exploded with great violence.

I placed the jar again in the garden during four hours longer in S 4 a fair

Quan-
tity of
the two
airs de-
stroyed.

a fair sun-shine, when I found the air still better by the nitrous test; for now one measure of it with one of nitrous air occupied 0.94; and it gave by Abbé Fontana's method 1.96; 1.87 $\frac{1}{2}$; 2.44; 3.40.

It had not, however, lost its explosive force.

116. Some of the pure inflammable air was put in a jar with an inverted plant of *Perficaria*, so that the root was in contact with the air; it stood during six days in the garden, when I found only $\frac{1}{5}$ of the air remaining, and this was no longer explosive nor inflammable, but a flame only grew dim in it. So that roots of water-plants have a remarkable power of absorbing inflammable air, as I found by several other experiments.

117. I

117. I gathered some inflammable air from stagnating water, by stirring up its muddy bottom. This air was so bad, that one measure of it with one of nitrous air occupied 1.98. One measure of this air was let up in a jar containing a sprig of pepper-mint, the root being cut off. It stood from ten in the morning till four in the afternoon in the sunshine; I found it so much corrected, that one measure of it with one of nitrous air occupied 1.60. It burnt as well as before.

118. An equal measure of the same inflammable air from the stagnating water was let up in a jar containing a small plant of *Persicaria urens*, with root and all. After standing the same time as the former, the air was examined, and I found more corrected than the other,
for

for one measure of it with one of nitrous air occupied 1.48; but it was as inflammable as before.

It seems to me probable, from the above-mentioned experiments, that plants have a power of correcting even the worst of all airs, inflammable air; but that they require some days to perform this transmutation, and that one and the same plant does not live long enough in full vigour to finish the business, if it is shut up in a narrow space with a certain quantity of this air; and that this air, after having been in a great measure mended by a plant, returns again to its former poisonous quality, if it remains with the plant after the vital operation of the plant ceases, which I apprehend was the cause of the difference of

of the event in Experiments 110, 111, 112, 113, 114, 115. Quantity of the two airs destroyed.

It appears also that plants have a power of changing inflammable air into a kind of air which is not to be known by the ordinary nitrous test, and which is the only air I know that explodes without the addition of any other air; so that it seems to be by itself a true *fulminating air*; for this inflammable air, after the *persicaria* plants were changed four times during 16 days, gave at last the following result, 1.81; 1.56; 1.37; 2.27; 3.25. 375

One measure of it with one of nitrous air occupied 0.84; with two measures of nitrous air 0.98; with three 2.00; and yet it had not lost its explosive quality, though by this diminution of its bulk with nitrous air it indicated to be far bet-

ter than common air, nay even to be dephlogistified air.

I make no doubt but the plants had communicated to this air the quality of being diminished by nitrous air, by mixing with it the dephlogistified air they yield of themselves; which is also the opinion of Abbé Fontana, to whom I communicated the experiment. But I cannot but think that the plants, by their vital powers, had changed this pure inflammable air into *fulminating* or explosive air, as this quality is given to it in one night, or in a dark place in a few hours; though plants yield no dephlogistified air in the night or in dark places, but scarcely any air at all, and whatever air they yield is phlogistified air, unfit for supporting flame. It even seems to me not improbable,

improbable, that living plants not only improve good air, or correct bad air, by communicating their dephlogisticated air to it, but also by a peculiar faculty they possess of purifying the circumambient air, which they may do by taking to themselves the inflammable particles, or by some other faculty they possess. Air fouled by breathing is thus rendered quite pure again in a few hours by a plant growing in it, as is already shewn above.

SECTION XVII.

Experiments towards investigating what plants or trees infect the surrounding air the least by night.

119. I PLACED in four different gallon jars an equal quantity, as near as I could, of leaves upon their stalks of the following trees, *lime-tree, oak, laurocerasus, walnut*. I placed all those jars over-night in a room, each inverted upon a dish; in each jar was as much water as would preserve the leaves alive by keeping the stalks wet. Next morning I found the air of all the jars contaminated: that of the walnut-leaves was become unfit for breathing, and extinguished flame;

flame ; that of the *laurocerasus* was next in foulness to the walnut ; then followed the lime-tree ; the oak had spoiled the air the least of all.

One measure of the air in which the walnut-leaves had been, with one measure of nitrous air, occupied 1.53 ; that of the *laurocerasus* 1.26 ; that of the lime-tree 1.16 ; and that of the oak 1.10.

120. I have observed that the branches of a vine generally infect the air much less by night than most part of other trees. Cabbage among the culinary plants was, of all I tried, the least disposed to contaminate air.

SECTION XVIII.

Experiments shewing that the purest dephlogisticated air, and the greatest quantity, is yielded by full-grown leaves.

121. I PLACED in a jar full of pump-water the extremity of a branch of a vine containing leaves of different ages, from the full-grown to those which begin only to unfold themselves. The bubbles appeared the first in the old leaves; and they broke out gradually upon the next in age; so that they appeared the latest upon the new-formed leaves. The same proportion takes place also in the size of the bubbles, as well as in the

the quantity of the dephlogisticated air obtained from them.

Quantity of
the two
airs de-
stroyed.

122. I placed in a gallon jar filled with water some old or full-grown leaves of a vine, and exposed it to the sun from nine in the morning till two in the afternoon, when a great quantity of very pure dephlogisticated air was obtained, whose test was 1.95; 1.85; 1.72; 1.60; 1.61; 2.53. 547

123. I placed in another jar of the same size a similar quantity of young leaves of the same vine, and exposed them to the sun during the same time. I obtained a good quantity of fine dephlogisticated air, but less, and of an inferior quality, than that obtained from the old leaves. Its test gave 1.97; 1.87 $\frac{1}{2}$; 1.78; 1.68; 2.33; 3.30. 470

SECTION XIX.

Experiments shewing that the sun by itself, without the assistance of plants, does not improve air, but renders it rather worse.

124. TWO jars, half-full of air taken from the atmosphere at the same time, and half-full of pump-water, were left by themselves during four hours, the one exposed to a bright sun-shine, the other placed within the house, only two steps from a door opening in the garden.

The air kept in the house gave, in six different trials, constantly the appearance of being better than that of the jar placed in the sun. One measure of the air kept within

doors with one of nitrous air occupied $1.06\frac{1}{2}$; whereas that exposed to the sun occupied $1.08\frac{1}{2}$.

I must however acknowledge, that this experiment ought to be repeated more than once, to put the fact out of any doubt. I made it the very last day of my stay in the country, and thus had no time to repeat it.

SECTION XX.

Experiments tending to investigate the most accurate and expeditious way of putting common air to the test, on purpose to judge of the salubrity of any country.

I HAVE already said enough, in the introduction to the second part of this work, of the accuracy with which this difficult and important investigation may be made by employing the instruments of Abbé Fontana; but as there is much more attention and dexterity required to judge with the greatest nicety of the degree of salubrity of the atmospheric air than of any other, as the other airs are of much less importance

importance to mankind ; I reserved this article for the last, not discontinuing to pursue my experimental enquiries till the book was already nearly printed off. Besides, it was but in the middle of September that I got the brass tube, expressed in Fig. I. in which the glass tube or great measure is suspended ; so that the column of water within and without the glass tube be at a perfect level, which is necessary to obtain a result constantly the same with the same air. This brass tube is a valuable addition to the apparatus.

I had before that time made experiments every day with the atmospheric air, placing the glass tube in a cylindrical jar filled with water, and lifting up the glass tube till the extremity of the column of

water within the tube was on a level with the brim of the jar; always taking hold of the glass tube by means of a piece of linen folded five or six times, and thoroughly imbibed with water, to prevent the warmth of my hand communicating itself to the glass tube.

Though I still think that Abbé Fontana's method of examining atmospheric air is the most accurate; yet, as I had it more in view to trace nature in the operation of vegetables than to examine the degree of salubrity of the common air, I endeavoured to abridge this trial as much as possible, on purpose to save time. For this reason, I got at last in the habit of making this trial in the time of a minute or two, and found a surprizing accuracy in the result. This mode is in some degree

degree composed of the methods used by the two most eminent philosophers in this branch of natural knowledge, the Rev. Dr. Priestley, and Abbé Fontana. It is this : I let up in the little measure as much common air as will fill it ; after which, I take hold of its brass slider, and keep it under water exactly 15 seconds, when I lift it up till the brass slider be on a level with the water of the trough, and shut the slider, to cut off the column of air within the measure ; I then invert the measure under water, to let out all the air which was remaining under the slider. I let up immediately this measure of air in the large tube, and fill the little measure in the same manner with nitrous air newly made from red copper, in the manner explained p.

171; which being also let up in the large tube, I begin to shake forcibly this tube in the water-trough exactly 30 seconds (beginning the motion precisely at the moment the two airs come into contact), and place it directly afterwards in the brass tube, and let it stand thus in the middle of the trough for the space of one minute, pouring continually water upon it, to bring the temperature of the glass tube to that of the water; for, holding it in the hand while shaking, it receives some degree of heat from the hand, and of course the column of air within is rarified. I then slide the glass tube up or down within the brass tube, which is filled with water, till the two columns of water come to a level with each other, and with the 0 of the brass measure,

measure, as is expressed in the plate by B.B in Fig. I. Then I observe with what number of the scale the first division of the glass tube above the column of water coincides, which shews me at once how many sub-divisions are remaining from the two measures of airs, or from the 200 subdivisions let up in the tube, and thus indicates the degree of goodness of common air, or indeed of any air approaching in goodness to common air, or being of an inferior quality. But this method will not do in examining dephlogisticated air, as this air requires more nitrous air to bring it to a full saturation. By this simple and easy method the whole operation is performed in three or four minutes ; and its accuracy is such, that frequently in ten trials, made
with

with the same common and nitrous air, the difference of the result does not amount to $\frac{1}{200}$ of the bulk of both airs.

The different degrees of salubrity will be found in general to lie between 103 and 109; at least, I found it almost always to be within these two extremes: that is to say, that of the bulk of the two airs the remaining column will be found to occupy between 103 and 109 subdivisions. The magnifier applied to the brass tube (D, Fig. 1.) assists greatly the accuracy of the observation.

This simple method constantly shewed me all the variations in the constitution of the atmosphere, in regard to its fitness for respiration, which I could discover by any other method.

A glass

A glass tube longer than that which I had at hand would bear a larger scale, and thus indicate with still more accuracy the goodness of the air : but the two measures of air let up must not fill more than one half of the glass tube, for otherwise it could not be shook in the water without danger of some bubbles of air coming out of it, or rushing in it, by the force of shaking it up and down.

In the works of the Rev. Dr. Priestley, one measure of common air is said to occupy sometimes 120, and even more subdivisions, which is owing to his peculiar method. He first joins the two airs together in a separate jar, and allows them to stand a certain time to incorporate one with the other ; after which, he lets them up in his large
tube

tube exactly divided, and sees at once, without any shaking, how much of the two airs is destroyed. If this method is pursued accurately, and if the same interval of time is observed between joining the two airs and letting them up, the result will, however, be found different in different experiments, as Dr. Priestley makes no scruple to allow.

I made a great many experiments to find out the reason of this difference; but this task I leave to Abbé Fontana, who commenced his enquiries on this subject prior to me. I will only relate one of my own, which will shew the reader what result he may expect from his experiments, though performed in the most regular manner.

I filled

I filled a jar with common air, and put one measure of it, with one of nitrous air freshly made, in five vessels, each of a different diameter, to incorporate with each other without moving them : after an hour's time, I let up the airs of the different jars into the large tube or measure, when I found that the column of air occupied so much the greater space as the vessel in which they stood was of a less diameter ; but none of these airs were diminished near so much as they were when shook immediately together in the way above-mentioned. It is very remarkable, that I could scarcely reduce any of these airs afterwards to a less bulk, though I shook them very forcibly in the large tube after I had examined them.

125. The common air used for this experiment proved to be of such a degree of goodness, that in six different trials, made one after another in the expeditious manner explained, the two measures occupied $1.06\frac{1}{2}$ exactly; whereas the same common air with the same nitrous air, after standing an hour in the five different vessels, gave the very different results expressed in p. 287.

By repeating the same experiments at different times, the same result was obtained as to the difference of the remaining bulk of airs kept standing in vessels of different diameters; but there was commonly a difference of some subdivisions even in the experiments made with the same glasses.

The

The bulk of the	And after
two airs kept in the	being hook
vessel of the largest	when first
diameter occupied in	examined
the glass tube with-	in the glass
out shaking	tube, $1.10\frac{1}{2}$,

That in the	
glass of the next	
in diameter	$1.23\frac{1}{2}$, 1.22

That in the	
glass of the next	
diameter	$1.28\frac{1}{2}$, 1.28

That in the	
following	1.35 , 1.35

That in the	
glass of the	
smallest diame-	
ter of all	1.44 , 1.43

P O S T-

P O S T S C R I P T.

AS I went on with my experiments during the whole time this book was printing, I continued to discover more and more the secret operations of nature in regard to cleansing our atmosphere. I have carefully registered in my notes the result of the experiments, which I may possibly communicate to the publick in a second volume, together with some more deductions which I may draw from my remarks.

Though I am obliged abruptly to stop my further researches, I cannot dismiss the reader without acquainting him, that, as soon as the warm weather began to cease, and the autumnal colds to set in (the thermometer of Fahrenheit being un-

der 50 in the shade, which had been in the time of the other experiments in general between 70 and 83), the leaves, fruits, and roots had lost a good deal of their mischievous influence upon the circumambient air in the night, and by day in shaded places, though they had lost nothing of their salubrious power in yielding by day dephlogisticated air; but that the flowers seemed to have lost very little or nothing of their malignant effluvia by which they contaminate the surrounding air; and that water standing by itself, or with plants in it, loses by the sun-shine, or rather by the warmth communicated to it in the sun, the faculty of promoting, or rather of not obstructing, the plants yielding dephlogisticated air; but that it recovers almost to an

U

equal

equal degree its former faculty, by the coldness of the night. Water, in which I found ice in the morning, and which the day before obstructed the leaves in yielding a tolerable quantity of bubbles, was so much recovered, when it was heated by the sun, that fresh leaves put in it yielded air-bubbles very briskly, when the thermometer plunged in it was at 37.

From what has been said in the nineteenth Section, as well as from other experiments, I am more and more induced to believe that our atmospheric air is a substance of a very changeable nature, and that it is, in common with a great many other substances, equally liable to become worse, or of undergoing a kind of corruption by the increase of heat ; and that this tendency to

3

corruption

corruption is checked by the vital operation of the plants in the summer, and by the cold in the winter. By this observation, we may perhaps be induced to believe, that those countries which are very hot in the summer, and are little or not cultivated, as is a great part of Hungary and the country round about Rome, are not only exposed to have their air contaminated by the breathing of animals in it, and by the corruption of many other substances, but also by the corruption which the air itself is liable to undergo during the heat of the season; and which mischief can chiefly be remedied by making a sufficient quantity of vegetables grow in them, principally trees. Draining the marshes, and preventing inundations by keeping the ri-

vers within their bounds by dykes, and by cutting canals to let out the waters, will greatly assist the operation of vegetables, which would be insufficient to cleanse the atmosphere of low countries, without this great cause of corruption, owing to marshes, being removed.

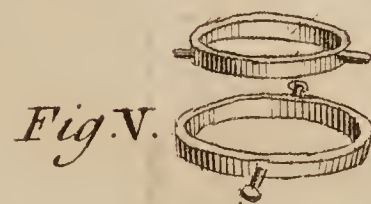
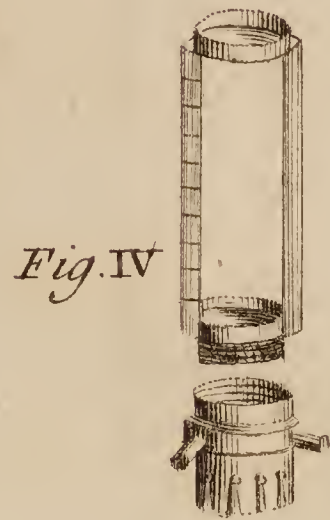
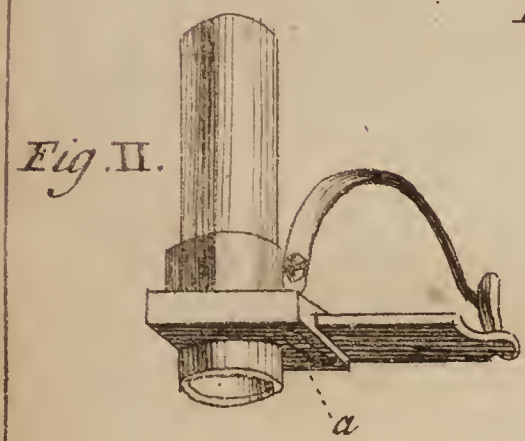
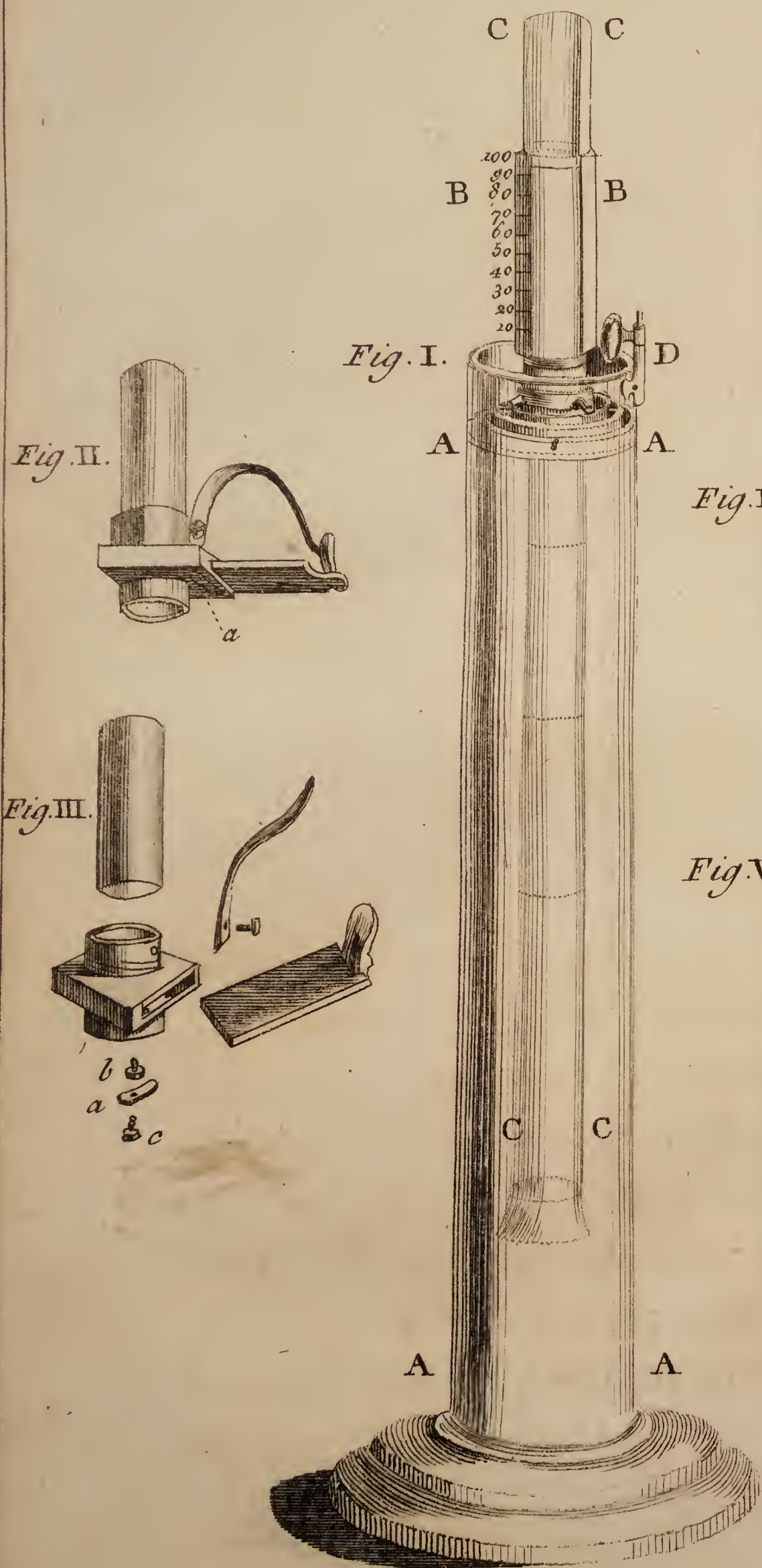


Fig. VII.

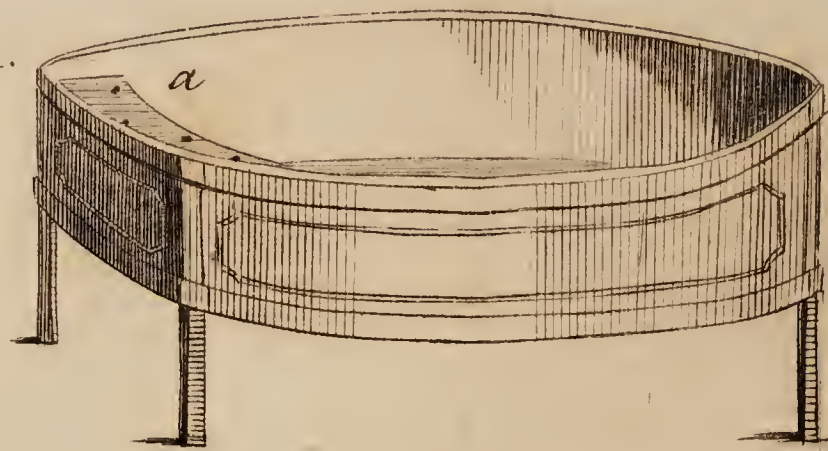


Fig. VIII.

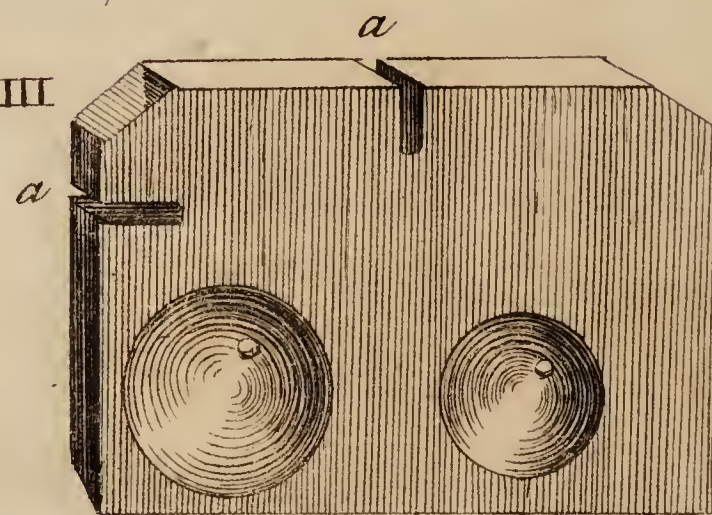


Fig. IX.



Fig. XI.

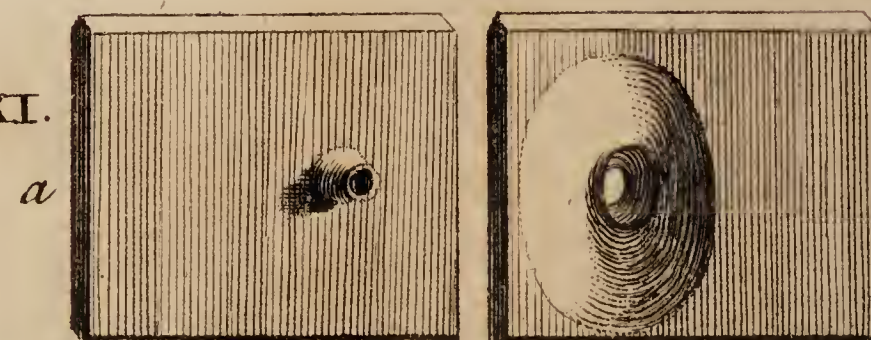


Fig. X.

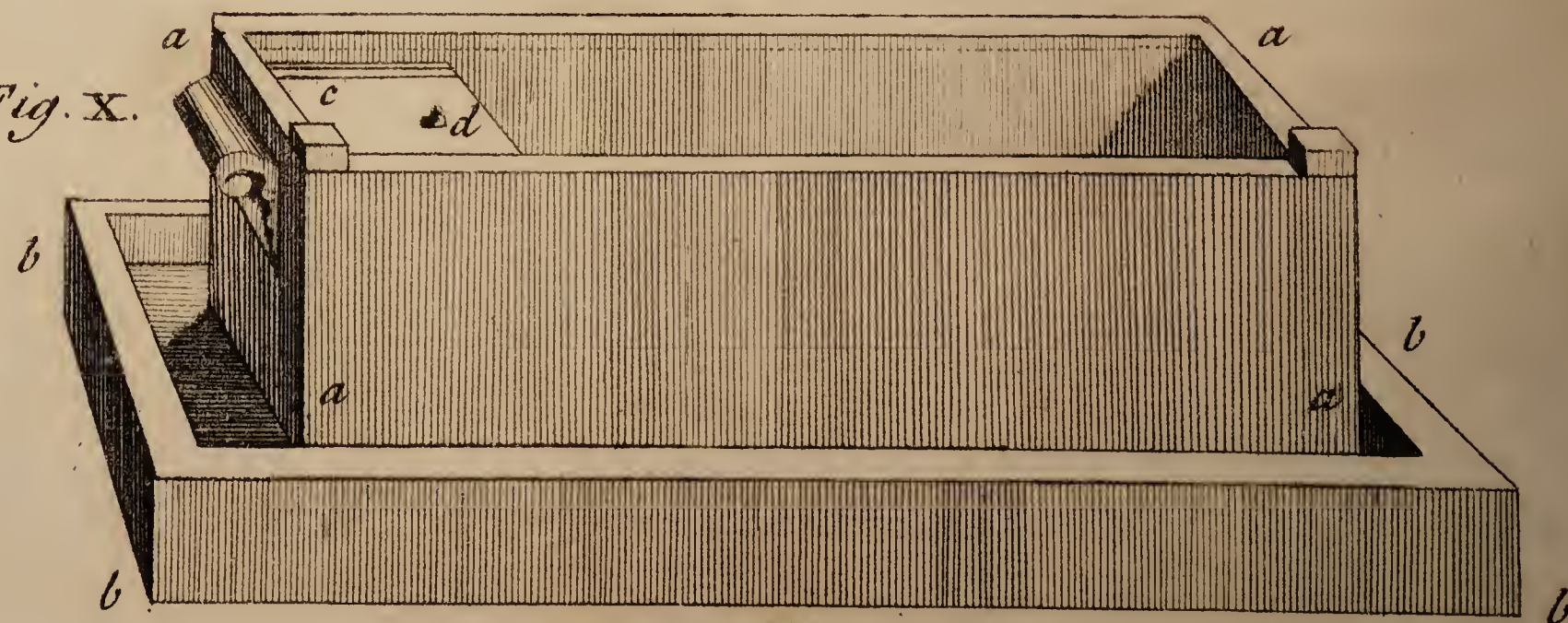


Fig. XII.



Fig. XIII.



EXPLANATION of the FIGURES.

Fig. I. THE great measure, or great glass tube, in its situation in the experiment of examining air, with the brass scale upon it, and the magnifier stuck to the brass tube, on purpose to adapt accurately the first mark of the scale to the beginning of the column of air. The glass tube is suspended upon the two brass rings or gingle (in the way the common sea-compasses are suspended) to keep it always in a perpendicular situation. A A A A is the brass tube full of water, in which the glass tube fixed to the brass measure is suspended. This brass tube is represented transparent on purpose to see in what manner

the glass tube is suspended in it. B B, the brass scale of three Paris inches divided into an hundred parts. C C C C, the glass tube or great measure, whose lower and open extremity is secured by a brass ferrule. D, the magnifier for the more accurate observation.

Fig. II. The small tube or measure fixed in its brass socket. *a*, is an elastic piece of brass, having a pin passing through a hole in the under part of the socket, which pin is pushed upwards by the elasticity of the piece *a*, and enters in a cavity on the under side of the slider, made on purpose to stop it, and to prevent the bended steel spring from forcing the slider entirely out of the socket. N.B. I think that this machine may do very well without

without the bended steel spring, and therefore I did not put it to that which I made use of.

Fig. III. The small tube or measure, with the brass socket, slider, and springs, all taken afunder to see their shape. The screw *c* under the elastic piece *a*, is to be fixed to the under part of the shoulder of the brass socket (at *a*, in Fig. II.), to fix the elastic piece *a* to it. The pin *b* supported by the piece *a*, and passing through a hole in the under part of the shoulder of the socket, enters a cavity made in the under side of the brass slider, when this slider is drawn out as in Fig. II. and thus stops its coming out entirely.

Fig. IV. The brass scale with its under-piece to be screwed to it, and serving to embrace closely, by its spring, the glass tube, so as to be suspended by it upon the brass gingle, expressed in Fig. V. The inside of this under-piece ought to be lined with a piece of sponge, on purpose to press softly against the glass, and to prevent its being scratched by sliding the glass tube up and down against the brass.

Fig. V. The gingle, or the two brass rings, such as are used in common sea-compasses, whose moveable axes act contrary to one another, to give the body suspended upon them every possible motion, and thus to keep it in a perpendicular line.

N. B.

N. B. I keep my tube suspended simply upon a brass ring, foldered a little way within the brass tube, which does very well.

Fig. VI. That part of the under-piece of the brass scale by which it is supported upon the gingle.

Fig. VII. The wooden-trough full of water, in which the whole apparatus is used. This trough ought to be (in the inside) 2 feet long, 13 inches deep, and 17 inches wide. The board *a*, upon which the jars, &c. are placed, ought to be fixed at the distance of $3\frac{3}{4}$ inches from the brim; the length of the board ought to be about 9 inches, the thickness of it two inches. The trough ought to be kept full of

of water except about two inches from the brim.

Fig. VIII. The board (expressed by *a*, in Fig. VII.) by itself, and inverted. It has two funnels hollowed out on its under-part, which is in this figure represented uppermost: the orifices of the funnels are represented by the two round holes, one of which must be larger than the other. *a a* represent two oblong incisions to receive the extremity of the bended tubes, through which the various kinds of air are let up into the inverted jars placed upon the board.

Fig. IX. A cut of the two funnels hollowed out in the board, represented by Fig. VIII.

The

The rest of the figures are only intended for those who should like to engage farther in this entertaining branch of natural philosophy, and to produce those kinds of air which are liable to be altered or absorbed by water.

Fig. X. A wooden trough to be filled with mercury, for such experiments with air as cannot be done in water. Many kinds of airs are absorbed themselves by water, as is fixed air, and all those aërial fluids which should rather be classed among vapours, as alkaline air, acid air, &c. of which an account may be seen in the works of Dr. Priestley, and which will soon be treated in a more ample manner by Abbé Fontana.

Air

Air extracted from spars cannot be examined, nor even obtained, but by making use of mercury instead of water: for this singular air, which corrodes glass, is immediately reduced into stone by the first contact with water.

This trough consists of two different strong wooden boxes. *a a* is the box containing the mercury; it is in the inside $11\frac{1}{2}$ Paris inches long, 4 inches and 2 lines deep, and 4 inches and 2 lines wide. The board *c* is placed at one inch and 2 lines distance from the brim, and is 7 lines thick. The orifice *d* of the funnel hollowed out of the under-side of this board is two lines above the surface of the board. This box is placed within another larger box equally strong, *b b b b*, which

which serves to receive the mercury spilt by moving the vessels in the other box.

Fig. XI. The board (represented by *c* in Fig. X.) of the box containing mercury, represented in *a* as it is fixed in the box; and inverted in *b*, on purpose to see the funnel hollowed out in the under-side of it.

Fig. XII. A section of the board of the box (represented by Fig. X. *c*), on purpose to shew the form of the funnel, and the manner of fixing this board in the box, by letting its sloping edges in a groove cut out in the substance of the box, so that the mercury cannot push it up, but that it may be taken out at pleasure.

Fig. XIII.

Fig. XIII. A kind of *forceps* or tongs, to receive the necks of different vessels, in which air is to be extricated by heat. It is fixed by the screw to the brim of the water-trough, or to the box of mercury; and the neck of the glass vessel is squeezed between the two branches by means of the moveable ring, by which they may be more or less squeezed together, according to the size of the neck of the glass. This instrument is very useful for different operations, which, without its help, would require an assistant to hold the glass to keep it from falling.

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* * p. refers to the number of the Pages ; and exp. to that of the Experiments.

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